Support from Plato that Amasis governed Egypt during the second half of the 5th century B.C.

In two of the latest of his works, the Critias and the Timaeus, Plato describes a discussion that purportedly occurred among four Greek notables, Critias, Timaeus, Hermocrates, and Plato's renowned teacher Socrates. According to the vast majority of scholars, this hypothetical discussion took place around the year 425 B.C., though some date it a decade or so later. Our interest in Plato's version of the conversation is restricted to a brief statement with chronological import, not with the story about Atlantis about to be introduced, nor with the several contentious issues related to the genealogy of Critias, the main character in this portion of the dialogue. It should be noted in passing that the majority of classicists identify this Critias as the famous Greek tyrant who bears this name, whose dates are roughly 460-403 B.C. We have followed that opinion here.

To set the stage, we quote the relevant introductory remarks in the conversation. Critias is speaking to Socrates, and is about to relate the story of the ancient conflict between Athens and the fabled city state of Atlantis. The reader must take care not to confuse Critias, the speaker, and his namesake grandfather Critias, the latter mentioned throughout the dialogue. Henceforth, for clarity, we will refer to the younger of the two as the tyrant Critias, and to his grandfather as the elder Critias. The quotation is taken from Vol. IX of Harvard's *Loeb Classical Library* edition of Plato's works, translated by R. G. Bury. The tyrant is speaking to Socrates.

> Crit. Listen then, Socrates, to a tale which, though passing strange, is yet wholly true, as Solon, the wisest of the Seven, once upon a time declared. Now Solon – as indeed he often says himself in his poems – was a relative and very dear friend of our great-grandfather Dropides; and Dropides told our grandfather Critias – as the old man himself, in turn, related to us – that the exploits of this city in olden days, the record of which had perished through time and the destruction of its inhabitants, were great and marvelous, the greatest of all being one which it would be proper for us now to relate both as a payment of our debt of thanks to you and also as a tribute of praise, chanted as it were duly and truly, in honour of the Goddess on this her day of Festival.
>
> Soc. Excellent! But come now, what was this exploit described by Critias, following Solon's report, as a thing not verbally recorded, although actually performed by this city long ago?
>
> Crit. I will tell you: it is an old tale, and I heard it from a man not young. For indeed at that time, as he said himself, Critias was already close upon ninety years of agel, while I was somewhere about ten; and it chanced to be that day of the Apaturia which is called "Cureotis." The ceremony for boys which was always customary at the feast was held also on that occasion, our fathers arranging contest in recitation. So while many poems of many poets were declaimed, since the poems of Solon were at that time new, many of us children chanted them. And one of our fellow-tribesmen – whether he really thought so at the time or whether he was paying a compliment to Critias – declared that in his opinion Solon was not only the wisest of men in all else, but in poetry also he was of all poets the noblest. Whereat the old man

(I remember the scene well) was highly pleased and said with a smile, "If only, Amynander, he had not taken up poetry as a by-play but had worked hard at it like others, and if he had completed the story he brought here from Egypt, instead of being forced to lay it aside owing to the seditions and all the other evils he found here on his return, - why then, I say, neither Hesiod nor Homer nor any other poet would ever have proved more famous than he." "And what was the story, Critias?" said the other. "Its subject," replied Critias, "was a very great exploit, worthy indeed to be accounted the most notable of all exploits, which was performed by this city, although the record of it has not endured until now owing to lapse of time and the destruction of those who wrought it." "Tell us from the beginning," said Amynander, "what Solon related and how, and who were the informants who vouched for its truth."

"In the Delta of Egypt," said Critias, "where, at its head, the stream of the Nile parts in two, there is a certain district called the Saitic. The chief city in this district is Sais – the home of King Amasis, - the founder of which, they say, is a goddess whose Egyptian name is Neith, and in Greek, as they assert, Athena. These people profess to be great lovers of Athens and in a measure akin to our people here. And Solon said that when he travelled there he was held in great esteem amongst them; moreover, when he was questioning such of their priests as were most versed in ancient lore about their early history, he discovered that neither he himself nor any other Greek knew anything at all, one might say, about such matters. And on one occasion, when he wished to draw them on to discourse on ancient history, he attempted to tell them the most ancient of our traditions, concerning Phoroneus, who was said to be the first man, and Niobe; and he went on to tell the legend about Deucalion and Pyrrha after the Flood, and how they survived it, and to give the genealogy of their descendants; and by recounting the number years occupied by the events mentioned he tried to calculate the periods of time. Whereupon one of the priests, a prodigiously old man, said, "O Solon, Solon, you Greeks are always children: there is not such a thing as an old Greek." Timaeus 20D-22B

At this point in the dialogue the Egyptian priest proceeded to introduce and then recount the ancient tale of the conflict between Athens and Atlantis, which ended with the utter destruction of the latter, not by human cause, but via a phenomenon of nature.

The balance of this story would, without doubt, be of immense interest to the majority of our readers, and is certainly relevant to our revision, since we conjecture in our history that Atlantis was *likely* the volcanic island of Santorini (otherwise known as Thera), inhabited by ethnic Minoans, and that its eruption around 765 B.C. not only destroyed the Minoan civilization, but also brought an end to the 19th dynasty of Egypt and the nation of the Hittites. Be that as it may, our interest here is restricted to two sentences in the section of dialogue quoted above, highlighted in the text. We repeat them here:

"In the Delta of Egypt," said Critias, "where, at its head, the stream of the Nile parts in two, there is a certain district called the Saitic. The chief city in this district is Sais – **the home of King Amasis**, - the founder of which, they say, is a goddess whose Egyptian name is Neith, and in Greek, as they assert, Athena. R.G. Bury: (1960)

Two additional translations of the quoted passage, both by well respected scholars from late in the 19th century, will suffice for our purposes.

> In the Egyptian Delta, at the head of which the river Nile divides, there is a certain district which is called the district of Sais, and the great city of the district is also called Sais, **and is the city from which King Amasis came**. The citizens have a deity for their foundress; she is called in the Egyptian tongue Neith, and is asserted by them to be the same whom the Hellenes call Athene. Benjamin Jowett (1871)
>
> There is in Egypt, said Kritias, in the Delta, at the apex of which the stream of the Nile divides, a province called the Saitic; and the chief city of this province is Sais, **the birthplace of Amasis the king**. The founder of their city is a goddess, whose name in the Egyptian tongue is Neith, and in Greek, as they aver, Athena R.D. Archer Hind (1888)

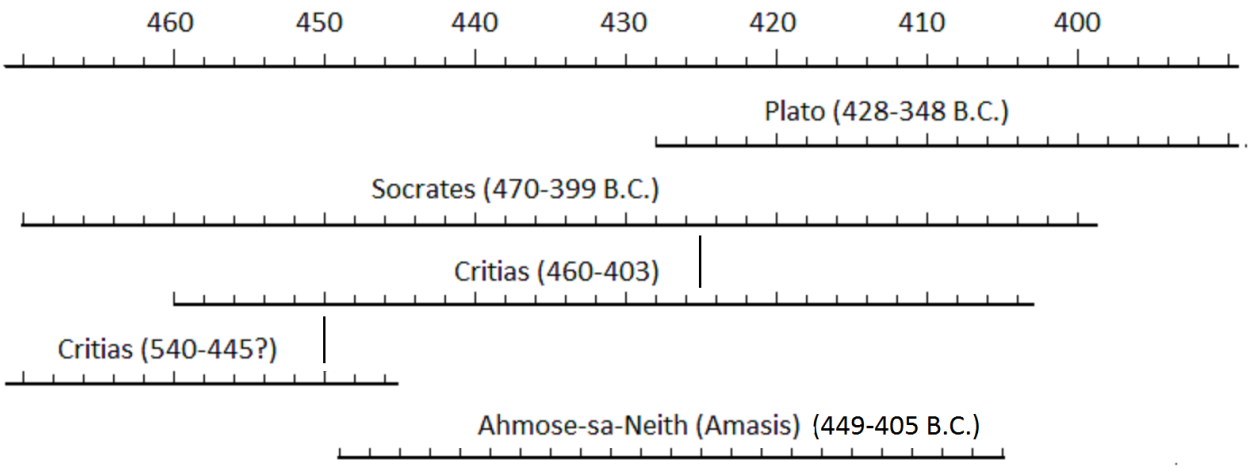
Although the speaker in this section of the dialogue is the tyrant Critias, he is, in this excerpt, quoting his grandfather, the elder Critias, who is speaking to a fellow tribesman named Amynander. The elder Critias is around 90 years of age, and listening attentively to his remarks is his 10 year old namesake grandson, soon to be known as the tyrant Critias. Those numbers, together with the reasonably assured dates of the tyrant Critias (460-403 B.C.), suggest that this conversation took place around the year 450 B.C., though considering Plato's utter disregard for historical accuracy, the date is not fixed in stone. The occasion, according to a footnote in the Harvard text, is the Apaturia, a feast held in October in honour of Dionysus on which children born during the year were registered.

From the highlighted section of the text it is clear that *around* the year 450 B.C. at least one Greek citizen is aware of the emergence in Egypt of a Saite dynasty king named Amasis. The elder Critias not only knows the Greek version of the name of the king, but appears to be aware of the kings full Egyptian birth name Ahmose-sa-Neith ("Ahmose son of [the goddess] Neith), or minimally, of the association of both Amasis and the goddess Neith with the city of Sais. And if one reads the text carefully, the elder Critias is not crediting Solon with this intimate knowledge. He is merely describing, for the sake of Amynander (and others listening in on the conversation), details concerning the city visited by Solon, whence came the knowledge about the conflict between Athena and Atlantis. Having completed this task he continues with a statement "And Solon said…" which introduces Solon into the story. Solon, for those unfamiliar with Greek history, was a 7th/6th century Athenian statesmen and poet noted for many accomplishments and later numbered as one of the 7 sages of Greece. His dates, according to most sources, were 638-558 B.C., though some date him slightly earlier (see http://en.wikipedia.org/wiki/Solon).

According to the elder Critias in the highlighted passage, the city of Sais visited by Solon is the "home of" or "birthplace of" or "the city from whence came" king Amasis. We ask the obvious question. If not from Solon, then whence did the elder Critias glean his knowledge about the city of Sais and its association with king Amasis and the goddess Neith. And why did he, and later also his grandson, repeat these facts if they would not in some way edify the listeners. After all, if neither Amynander in 450 B.C nor Socrates in 425 B.C. had a clue who Amasis was,

the comments would cause more confusion than clarification. And we ask additionally, assuming the accuracy of the traditional history which dates the reign of Amasis in the years 570-526 B.C. – how is it that both the elder and the tyrant Critias and their audience (including Socrates) knew anything at all about an obscure Egyptian king, now deceased for over a century? Would the conversation not make more sense if we were to assume that everyone privy to it was familiar with the name of king Amasis, because Amasis was alive and well and presently governing in Egypt. In the revised history Amasis ruled Egypt in the time frame 449-405 B.C. The vertical lines in Figure 1 below indicate *roughly* when the Atlantis story was passed from the elder Critias to Amynander and his grandson, and thence, two to three decades later, to Socrates, at roughly the halfway mark in the reign of Amasis.

Figure 1: Timeline of the conversations in Plato's Timaeus.



Considering that the comments concerning Egypt were transmitted to audiences *around* 450 B.C. and 425 B.C. respectively there is every reason why the references to Sais, king Amasis, and even the goddess Neith would have been comprehended by the respective audiences. In considerable detail the first book in the Displaced Dynasties series describes the chaotic power struggle in which the Persian backed interloper Amasis replaced the Saite king Apries (whose throne name was Amyrtaeus). That contest for power lasted from 454-449 B.C., with the Greek navy backing Apries, while Persia and the Egyptian army sided with Amasis. Details of the machinations of that civil war can be read in the 10th chapter of *Nebuchadnezzar & the Egyptian Exile* (pages 318-348), the 1st volume of the Displaced Dynasties series. That chapter, entitled "Amasis & the Greeks", depends largely on the historical treaties of Thucydides, Ktesias, and (with an extreme degree of caution) Herodotus, aided by the Elephantine (Amasis) Stele, referred to by the Egyptologist Breasted as "perhaps the most important document of the Saite

period". Here we learn that although the Greeks first sided with Apries, their allegiance changed once Amasis was declared king in 449 B.C. Thereafter, and throughout the reign of Amasis, commerce between Greece and Egypt flourished, with the port city of Naucratis, not far from Sais, as the focal point. This commerce, encouraged in no small measure by the passage of the "Peace of Callias" (http://en.wikipedia.org/wiki/Peace_of_Callias), also in 449 B.C., drastically changed the attitude of the Egyptians toward the Greeks. In the opinion of this author, the single sentence which follows the highlighted text initially quoted in this article, and which immediately precedes the introduction of Solon, reflects this changed attitude.

> "These people [the Egyptians] profess to be great lovers of Athens and in a measure akin to our people here.

We cannot leave this matter without noting the one objection certain to be raised. There is a segment of the scholarly community that attributes the whole of the discussion vis-à-vis Egypt to Solon, essentially negating our argument. The reader will understand immediately why scholars might adopt that position. After all, there is to my knowledge not a single academic familiar with the traditional history of Egypt who does not accept the dates 570-526 B.C. for Amasis (with the single exception of those who have accepted the author's DD hypothesis). And since Solon is dated ca. 638-558 B.C. there was ample opportunity for him to add the words concerning Amasis and Sais to his discourse on the subject of Atlantis toward the end of his life, regardless of when he visited Egypt. In the final analysis it will be up to the reader to evaluate whether our thesis is convincingly argued, given the language of the text, the fortuitous timing of the discourses, and the fact that, apart from Herodotus, whose history with respect to Egypt is almost a complete fiction, Greece and Egypt are not known to have had any special affinity toward one another in the 6th century.

And speaking of Herodotus, we should mention one last possible objection. Twice in the book which bears his name, the text of Herodotus actually mentions Solon and Amasis in the same context, leaving no doubt that Amasis lived in the 6th century where he may well have been visited by the Athenian sage. How do we respond to these anachronisms?

The first such text is found in Book 2, section 177. We actually quoted this text in our Volume 1, p. 333, stopping short of the mention of Solon. We quoted it because we believe the text accurately describes Egypt in the time of Amasis, albeit an Egypt ruled by Amasis in the 5th century, not the 6th. We quote the passage again here, but this time in its entirety.

> It is said that in the reign of Amasis Egypt attained to its greatest prosperity, in respect of what the river did for the land and the land for its people: and that the whole sum of inhabited cities in the country was twenty thousand. It was Amasis also who made the law that every Egyptian should yearly declare his means of livelihood to the ruler of his province, and, failing so to do or to prove that he had a just way of

> life, be punished with death. *Solon the Athenian got this law from Egypt* and established it among his people; may they ever keep it! For it is a perfect law. (emphasis added)

According to this passage Herodotus dates Amasis' lawmaking activity in Egypt prior to that of Solon in Athens. But even in the context of the traditional history that is an impossibility. Solon lived from 638-558 B.C. and his political activity in Greece took place while he was middle aged, several decades before Amasis began to rule Egypt in 570 B.C. For that reason alone, the mention of Solon in this context is clearly a late addition to the text of Herodotus. But there is a more fundamental reason for disregarding this association of Amasis and Solon. In our first book we have already argued at length that the second book of Herodotus is a corrupt late addition to the existing work of the 5th century Herodotus. We attributed the work to a 4th or even a 3rd century imposter whom we labelled the "pseudo-Herodotus". The 2nd book of Herodotus has mistakenly telescoped two invasions of Egypt, one which took place shortly after the death of Amasis in 405 B.C., and one, led by Cambyses in 525 B.C. As a result the Egyptian 26th Saite dynasty has been unceremoniously wrenched from its 6th/5th century context and positioned in the 7th/6th centuries, prior to the arrival of Cambyses in 525 B.C. The interested reader can peruse the online copy of our Volume 1, where we devote two entire chapters to proving our case: chapter 6 (pp. 173-206) entitled "Cambyses in Egypt" and chapter 11 (pp. 348-372) entitled "A Second Cambyses". Herodotus chapter two is not totally bogus, but where it assumes the 26th dynasty lies in the time frame 664-525 B.C. it must be ignored.

The second corrupt reference to Solon and Amasis in the same context is found in Herodotus Book 1, sections 29 and 30. In a sense, if accepted as valid, this innocuous mention of Amasis in the 6th century would be more fatal to our thesis than the first. Here we have Solon actually visiting Amasis in Egypt, and this in a section of the *Histories of Herodotus* that was arguably composed by purported author.

In Book 1: 29-30 we read, concerning a visit of Solon to Croesus, the Lydian king …

> There came to the city [of Sardis] all the teachers from Hellas who then lived, in this or that manner; and among them came Solon of Athens: he, having made laws for the Athenians at their request, left his home for ten years and set out on a voyage to see the world, as he said. This he did, lest he should be compelled to repeal any of the laws he had made, since the Athenians themselves could not repeal them, for they were bound by solemn oaths to abide for ten years by such laws as Solon should make.
>
> For this reason, and to see the world, Solon left Athens *and visited Amasis in Egypt* and Croesus at Sardis; and when he had come, Croesus entertained him … (emphasis added)

And the text goes on, page after page concerning Croesus, ending only in 1:56. The entire discourse is concerned with Croesus, not Amasis. The lone mention of "Amasis in Egypt" in a context totally devoted to a discussion about Croesus in Lydia is arguably a gloss, added to the text in the 3rd or 4th century, when the pseudo-Herodotus composed and amplified the History

of Herodotus with the addition of the 2nd chapter and some third chapter material. The reference may well have been gleaned from the published work of Plato's Timaeus, wrongly interpreted. If that claim seems incredible to the reader, we must add the observation that even the Croesus material is clearly a fabrication, this by Herodotus himself. A simple reading of the text should suffice to convince the reader that the material is fictional. And we can easily dismiss it on historical grounds. Croesus did not begin to rule Lydia before 560 B.C. Solon died in 558 B.C. and his lawmaking in Greece took place around 600 B.C. Only a flight of fancy of the part of Herodotus can explain the content in 1:29-56. But lest we be accused of a biased opinion, we should quote from no less an authority than W.W. How and J. Wells in their classic 1929 "*Commentary on Herodotus*", where the authors state on page 66:

> The truth of his story as to Solon and Croesus was early doubted, and it is now universally given up, on chronological grounds.

And if a dozen pages of text regarding Croesus is to be abandoned, what alternative do we have but to trash the "and … Amasis in Egypt" addition.

With that we close our argument. As always, LET THE READER DECIDE.

Solomon, king of Israel and his contemporary, Amenhotep III, pharaoh of Egypt

Throughout the four books of the Displaced Dynasties series we have argued repeatedly concerning the Egyptian 18th dynasty that many of its kings had namesakes in the 25th dynasty, a fact that Egyptologists recognize in theory but not in practice. As a result the accepted history of both dynasties is hopelessly muddled. Fortunately for the discussion which follows, Amenhotep III, the ninth king of the 18th dynasty, is one of the exceptions to the rule. To our knowledge he had no 25th dynasty counterpart. All monuments and documents which bear his name legitimately belong to him and him alone. And the traditional history based on the interpretation of these documents is undoubtedly correct, at least insofar as that interpretation does not rely upon or comment on past and future historical events. The one objection the present author raises against the interpretation of Amenhotep's illustrious 38 year reign is that it did not occur in the time frame 1391-1353 B.C. (or thereabouts). The dates for Amenhotep III are roughly 966-928 B.C. Historians are in error by an astronomical 425 years!

The revised dates for Amenhotep III were discussed briefly in chapter 2 of the third volume of the Displaced Dynasties series. In table 4 on page 21 of "*The Genealogy of Ashakhet Part 1: From Amarna to Troy*" we summarized the results of our analysis of one segment of the Egyptian history preserved on the Berlin stele, an indispensable monument in our reconstruction of the Egyptian dynasties 11 through 19. For the convenience of the reader that table is reproduced below.

Based on the Berlin Stele we were able to determine approximate dates for the terminal kings of the 18th dynasty. Those dates were summarized in table 5 on page 25 of the same chapter, and they are also reproduced below.

In is important that the reader recognize that much of the Displaced Dynasties chronology of Egypt is based on the Berlin Stele. Dates of the Egyptian kings from the 11th through to the end of the 19th dynasties (i.e. from the 15th through to the mid-8th centuries B.C.) were determined from this single monument, which summarizes the carefully preserved records of the priests of Ptah in Memphis. We have not contrived our dates to suit some preconceived history of Egypt. They are, so to speak, "the hand we were dealt." If they are found to be in error in any single dynasty, because they are in conflict with Egyptian history known from independently dated sources, then the whole of the revised chronology will need to be reevaluated.

Table 1: Data summarized from the 2nd and partial 3rd row on the Berlin Stele (copy of Table 4, page 21, Volume 3)

Position Number	High Priest/Prophet Named	Name of King Served	Approx. Date of Birth	Approx. Date of the H.P./Prophet
2.1	Ptahemakhet	not named	808 B.C.	773 B.C.
2.2	Neferrenpet	Ramses II	824 B.C.	789 B.C.
2.3	Ptahemakhet	Ramses II	840 B.C.	805 B.C.
2.4	[--]masjemet?	Ramses II	856 B.C.	821 B.C.
2.5	Ptahhotep	Ramses II	872 B.C.	837 B.C.
2.6	Neterwihotep	Menmaatre (Seti I)	888 B.C.	853 B.C.
2.7	Sekeremsaef	Menmaatre (Seti I)	904 B.C.	869 B.C.
2.8	Ty	Tjeserkheperre (Haremheb)	920 B.C.	885 B.C.
2.9	Sekeremsaef	not named	936 B.C.	901 B.C.
2.10	Aypew	Ietnetjeray (?) (Ay)	952 B.C.	917 B.C.
2.11	Wirmer	Nibmaatre (Amenhotep III)	968 B.C.	933 B.C.
2.12	Penpanebes	Nibmaatre (Amenhotep III)	984 B.C.	949 B.C.
2.13	Nekhememptah	not named	1000 B.C.	965 B.C.
2.14	Nebre	Menkheperre (Thutmose III)	1016 B.C.	981 B.C.
2.15	Damaged section		1032 B.C.	997 B.C.
3.1	Ty	not named	1048 B.C.	1013 B.C.
3.2	Pa'emrud	Djeserkare (Amenhotep I)	1064 B.C.	1029 B.C.
3.3	Ty	not named	1080 B.C.	1045 B.C.
3.4	Menet	Nebpetire (Ahmose I)	1096 B.C.	1061 B.C.

Table 2: Summary of dates for the late kings of Dynasty 18 (copy of Table 5, page 25, Volume 3)

Name	Regnal Years
Amenhotep III	966-928
Amenhotep IV (Akhenaton)	940-923
Smenkhkare	923-920
Tutankhamun	923-914
Ay	918-914

Two arguments, one simply a review of material already presented, and one examining new material directly concerned with the reigns of Solomon and Amenhotep III, combine to establish the fact that Amenhotep III (966-928) and Solomon (970-930) were contemporaries. The first of these argument, that which is based on the preceding Egyptian history, will be summarized briefly below as Argument 1. The second Argument deals exclusively with the military capacity and activity of the two kings.

Argument 1: The revised history which postdates the time of Solomon firmly establishes the Amarna age of Egypt in the time frame 930-910 B.C. Since the later years of Amenhotep fall in this time frame, his regnal dates have already been firmly established.

The dates provided via our analysis of the Berlin Stele have already been utilized through the whole of book 2 of our Displaced Dynasties series. In particular, the dates 840-774 B.C. for Ramses II were derived using the same methodology used in extending the Berlin Stele dates backward in time through dynasty 18. By the time we determined the dates for Amenhotep III (chapter 2, page 24) we had already argued convincingly that the dates for Ramses II and Seti I of the 19th dynasty, and for Horemheb and his predecessors in the so-called Amarna age (930-900 B.C.) were extremely accurate. In the case of Ramses II the dates 840-774 were verified, in part, by comparison with the dates for the Hittite kings, with whom Ramses had frequent intercourse. Closer to home, the dates for the Amarna kings were discussed and verified via a lengthy discourse concerning the rebel king Labaya, mentioned frequently in the Amarna correspondence. In chapter two of Volume 3, in a section entitled "The Amarna Age" (chapter 2, pages 24-43), we argued at length that Labaya must be identified as Jereboam I, who along with Rehoboam, the son and successor of Solomon, ruled separate divisions of Solomon's kingdom. We have no intention of reviewing the argument here. We leave it to the interested reader to peruse and digest the relevant pages. Suffice it here to reproduce the closing addendum to that twenty page analysis.

> Addendum (reproduced from page 43 of chapter 2 of Volume 3)
>
> At minimum the preceding discussion allows the possibility that our theory regarding the Berlin genealogy chronology is correct. If we are wrong then even the staunchest critic of our revision must admit that we have been most fortunate. The Berlin chronology for the late 18th dynasty was nothing more than an extrapolation of the genealogy already applied successfully to the 7th century revisions of our earlier book and to the 8th century revisions proposed in the first chapter of the present book. This extension compelled us to place the Amarna correspondence roughly in the time frame 930-910 B.C., the time of the division of the Israelite kingdom. We had no choice in the matter.
>
> Consequently, had we not already read the Amarna letters from the Palestine region, we would have expected to find in them reference to a rebel king, residing in Shechem and in control of the entire region north of that city, ranging from the Mediterranean to the trans-Jordan. We would have expected that he would be in conflict with Megiddo and Gezer and Jerusalem, the cities in the southern region fortified by Solomon several decades earlier and now held by Rehoboam. We would have anticipated that Egypt, the former sanctuary of the rebel king, and more recently the conqueror of Jerusalem, would have an established presence in the area. And we could have predicted that Egypt would be siding with the rebel king, considering his previous liaison with the Egyptian court, even if we discounted the midrash contained

in the Septuagint regarding a marriage between him and pharaoh's sister-in-law. We would have demanded that the city of Rehoboam be named Jerusalem and that ethnic references to the rebels, if any were given, would allude to them as Hebrews (Habiru). There is nothing in the Jewish historical books describing the actions of Jeroboam and Rehoboam that conflicts with the data in the Amarna letters, in spite of the fact that many dozens of letters are forthcoming from the region of Palestine. The reader can believe, if he/she so chooses, that this is merely a fortunate coincidence. Or he/she can accept the ever increasing volume of data, by now filling well over 700 pages of closely reasoned text in two books and the beginning of a third, arguing for the accuracy of our revised chronology.

Argument 2: The initial sentence of the last paragraph gives us some direction as to how to proceed in formulating a more direct argument, based solely on activities related to the reigns of Solomon and Amenhotep III (and to a lesser extent his co-ruler Amenhotep IV). Our intent is to peruse the chapters of the Hebrew Bible which deal with the reign of king Solomon, and from our observations describe what would likely, or even necessarily, be the state of affairs in Egypt at that time, at least regarding the military situation in each country. Confirmation that in fact our conclusions concerning Egypt are precisely what prevailed during the reign of Amenhotep III would offer valuable support for our thesis and would at minimum prove that there is no known obstacle to our relocating Amenhotep's reign to the time of Solomon.

While we might easily have documented in this section the remarkable similarity between the two kings vis-à-vis their unprecedented building activity, their enormous wealth, with emphasis on their possession of hundreds of tons of gold, apparently from newly discovered sources, and their penchant for accumulating enormous harems, and cementing diplomatic relations with other counties through marriages of convenience with daughters of foreign dignitaries, we leave that activity for others. Needless to say the similarities between the two kings in these areas are quite remarkable, so much so that at least one well intentioned but misguided scholar named Ahmen Osman, while leaving the two kings in their traditional contexts, separated in time by hundreds of years, has opined that "the inescapable conclusion is that the story of Solomon was patterned specifically after the life of Amenhotep III" (House of the Messiah, p. 218) We do not recommend reading Osmon's works, but the interested reader may be interested in viewing a one page online summary of Osmans data produced by Charles Pope, (see http://www.domainofman.com/ankhemmaat/solomon.html).

We now divide our attention between two aspects of the military of the two kings. In section A we discuss the military actions (or rather the lack of such) in the two countries. In section B we take a brief look at the two sections of the respective armies, the "foot soldiers" and the "horse & chariot" brigades

Part A: The actions of the military in Solomon's United Kingdom and the Egypt ruled by Amenhotep III (and his co-ruler Amenhotep IV).

The story of Solomon is told in the Hebrew Bible in 1 Kings 2:12-11:43 and 2 Chron. 1:1-9:31. For the story of Amenhotep's Egypt we restrict our quotations (primarily though not exclusively) to two recent volumes from the corpus of fine synopses of his reign, in this case the "Chronicle of a Pharaoh: The Intimate Life of Amenhotep III" authored by Joann Fletcher (Oxford University Press, 2000) and "Amenhotep III, the Radiant Pharaoh" authored by Arielle P, Kosloff (Cambridge University Press, 2012).

Military Activity in Israel

It is absolutely clear from the text of the Hebrew Bible that Israel was at peace during the whole of Solomon's reign, and that for that entire duration Solomon controlled the whole of present day Syria and Palestine, from the Euphrates to the border of Egypt

> Now Solomon ruled over all the kingdoms from the River to the land of the Philistines and to the border of Egypt; they brought tribute and served Solomon all the days of his life. (1 Ki. 4:21 cf. 2 Chron. 9:26)
>
> For he had dominion over everything west of the River, from Tiphsah even to Gaza, over all the kings west of the River; and he had peace on all sides round about him.
> So Judah and Israel lived in safety, every man under his vine and his fig tree, from Dan even to Beersheba, all the days of Solomon. (1 Ki. 4:24,25)
>
> You know that David my father was unable to build a house for the name of the LORD his God because of the wars which surrounded him, until the LORD put them under the soles of his feet.
> But now the LORD my God has given me rest on every side; there is neither adversary nor misfortune. (1 Ki. 5: 3,4)

From these texts it should be evident whatever Egyptian realm or realms we position alongside the forty year reign of Solomon, there cannot be in evidence any aggressive activity north of the border of Egypt.

Military Activity in Egypt

Considering our previous statement it is instructive, and perhaps the most powerful argument in defense of our thesis, that Amenhotep III, in spite of possessing an expansive, well trained and well equipped military, did not engage in a single land campaign north of the country. In fact, the only recorded campaign by Amenhotep took place early in his reign, the 5th year to be specific, and that was a minor campaign launched against Nubia, hundreds of miles upriver (south) of Thebes. If Solomon's reign can be styled one of "peace and prosperity", it is not surprising that Fletcher entitles one section of her 3rd chapter: "*Pax Amenophica: Peace and Plenty*", and states that:

> By the thirteenth year of the reign, with Nubia stabilized and the vast empire at peace, Egypt was at the height of its wealth and power. *The rule of Amenhotep III saw four decades of prosperity uninterrupted by war*: for the people of Egypt it was a time of unparalleled security and optimism – a golden age presided over by a golden king. (p. 76) (emphasis added)

Fletcher's comments are echoed by a multitude of well researched sources on the internet, some nameless, one of which, chosen at random, adequately sums up our argument in this section. When we move Amenhotep forward in time by hundreds of years at the direction of the Berlin stele, and to a position uniquely determined by the relocation of the 18th dynasty kings who followed him, we appear to have found one of the few time frames (if not the only time frame) where he could have been positioned. How fortunate!

> Before Amenhotep III rose to the throne as Pharaoh of the Egyptian Empire, it was a place of tumultuous fighting for his ancestors; he however, was to live in an Empire of peace and enjoy the delights of the Ancient world. "*Through wars, his predecessors had established an empire; it was Amenhotep's destiny to enjoy its fruits and live off his ancestor's achievements.' (Hallo & Simpson- The Ancient Near East: A History)*. When Amenhotep III succeeded the throne of Egypt, his Empire was the unrivalled leader of the known world. After the multitude of campaigns launched by his predecessors, Amenhotep rose to power with little disruption from his neighboring nations. Gold poured in from Nubia, cedars came from the forests of Syria-Palestine and within Egypt, a huge supply of luxurious foods were coming from the fertile banks of the Nile. With his extensive power and wealth Amenhotep was able to launch the greatest building program the world had ever seen. Craftsman within his reign were able to display such scenes of artistic magnificence, which were never to be equaled in the 1500 years of Egypt's Pharaohs.
> http://amenhotepiii.weebly.com/index.html

> It was essential in any Pharaoh's reign to present themselves as a warrior and protector of Egypt; this was in order to uphold their duty in maintaining *maat* throughout their Kingdom. In the reigns of Amenhotep III's predecessors this role was carried out with battles and campaigns to the various neighbors of Egypt, such as Syria-Palestine, Nubia and Babylon. However, in Amenhotep's reign this was not fulfilled like in years gone by. This was due to the peaceful relations that Egypt now held with its neighbors; the need for war was unnecessary. This of course left the army with little occupation, something the Pharaoh put to his own use in his administrative structure. Although the Pharaoh only made one military campaign in Year 5 of his reign, the King still presented himself as a warrior, hunter and protector of Egypt, as was custom.
> http://amenhotepiii.weebly.com/the-warrior-pharaoh.html

The reader will no doubt agree that these two paragraphs could well have been describing the situation in Solomon's kingdom.

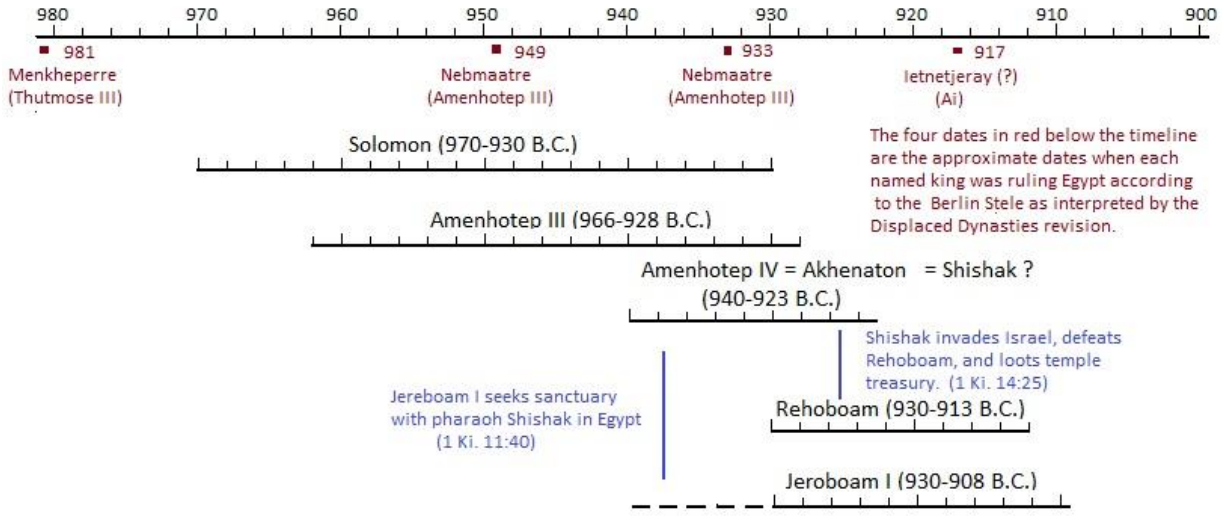
Before moving on to document the almost identical military resources of each country, we need to caution the reader about comments they are likely to read online about Egyptian dominance along the Mediterranean coastline during the reign of Amenhotep III. Nothing could be further from the truth. The source of this misrepresentation is the Amarna archives, as misinterpreted by scholars. Among this treasure trove of letters there are many written concerning the aggressive actions of an Amorite chieftain named Abdi-Ashirta and in particular this rebel chieftain's assault against a certain Rib-Adda of Byblos. The beginnings of this activity arguably took place extremely late in the reign of Amenhotep III, when Solomon was dying, but still alive. We do not doubt for a moment that immediately following the death of Solomon, the Egyptian presence along the Mediterranean coast would expand considerably and rapidly, but in the earliest documents he appears to be living. From later Amarna documents Fletcher learns that three Egyptian administrative areas have been set up, in Gaza (Canaan), Kumidu (Lebanon) and Simurru (Syria), but from these letters one cannot conclude anything about the reign of Amenhotep III. Most of this Amarna correspondence dates from the reign of Amenhotep IV (and later), but Fletcher argues from it that these areas were vassal states of

Amenhotep III, and from this conclusion she extrapolates and turns Amenhotep's realm into a "great empire" (p. 74). In truth, Amenhotep ruled nothing beyond the borders of Egypt. Even Nubia was not demonstrably an Egyptian province.

The reader needs to be clear that the reign of Amenhotep extends at most two years beyond the death of Solomon, and Egyptologists agree that during those years he was ill and his son and eventualy successor Akhenaton was the de facto ruler of the country. It follows that any expansion of Egypt toward the north, as reflected in the Amarna letters, is not in conflict with our thesis. This expansion is, in fact, an expected consequence of Solomon's death.

We provide below (Figure 1) a timeline to add some clarity to these comments, and to assist our description in arguments to come.

Figure 1: Timeline of the reigns of Solomon, Amenhotep III, and their successors



This latter discussion is not entirely defensive posturing on our part. There is some positive argument that can be drawn from the timing of the events. In the earliest letters that refer to Abdi-Ashirta it is interesting that the rebel appears to be dealing with habiru, whom we believe to be Hebrew officers, part of a vast network of administrative officials, numbering in the thousands, installed by Solomon and acting on his behalf. We know from the Hebrew Bible that Solomon's rule extended northward to the Amurru area, where Abdi-Ashirta ruled, and from whence Solomon's officials "brought tribute and served Solomon all the days of his life." (1 Ki. 4:21). Apparently, in these letters, the realm of Solomon remains intact, and tribute is still being collected. But, as stated above, these few Amarna letters are being misinterpreted by scholars. In later Amarna documents Solomon is clearly deceased. Abdi-Ashirta is acting more independently, and his adversaries are Egyptian. We quote a "brief and to the point" online document authored by a Robert McRoberts, who, like Fletcher, has also misread the situation, but who, unlike Fletcher, at least separates the two scenarios. And McRoberts has conveniently

provided a map to enable readers unfamiliar with the territories mentioned. We remind them that the one date mentioned (1350 B.C.) with which McRoberts begins his comments, is based on the traditional history, in which Amenhotep ruled in the years 1391-1353. He has only a few years left in his reign, and he is not well. In the first paragraph quoted Solomon appears to be still living; in the second he is dead and Egypt rules the day.

> The Amorite Chieftain Abdi-Ashirta had gained control of the land of Amurru around 1350 B.C.E. while ostensibly acting in good faith as a vassal of the Egyptian Pharaoh Amenhotep III and possibly Amenhotep IV/Akhenaten as well. Over a period of a few years Abdi-Ashirta increased his influence over a group known as the habiru, who appear to have been a mix of tribal peoples and outlaws. By rewarding the habiru with plunder from his conquests Abdi-Ashirta was able to quickly expand his territory and his base of followers.
>
> After consolidating the rugged hinterlands and valleys of Amurru, Abdi-Ashirta moved against the main Egyptian stronghold in the region. In a bold move he occupied the port city of Sumur, which had previously been the seat of the Egyptian commissioner. Then the ambitious warlord began to move south against the territories of Rib-Hadda the Egyptian vassal king of Gubla/Byblos.
> (https://suite.io/robert-mcroberts/3ae12z5)

Since we have mentioned the fact that Amenhotep was not well at the end of his reign, we should perhaps justify and qualify that statement. The mummified remains of Amenhotep III were discovered in 1898 in tomb KV35, west of Thebes. The corpse was deposited among a cache of corpses hidden in the tomb of his grandfather Amenhotep II, but "it was so badly damaged that even his age at death was difficult to determine". In 1912 the body was examined by the English anatomist Grafton Elliot Smith who "estimated that at the time of death Amenhotep III was probably "between forty and fifty years of age". The mummy shows that "Amenhotep III spent the last years of his life as a fat, diseased, sedentary man, almost completely bald", and was doubtless "in considerable pain from dental problems". The quotations are all taken from James E. Harris and Kent R. Weeks, *X-Raying the Pharaohs*, p. 142.

Part B: The military resources in Solomon's United Kingdom and the Egypt ruled by Amenhotep III (and his co-ruler Amenhotep IV).

The Military of Solomon:

In the case of Solomon the biblical text is our only source. We quote the most relevant passages.

> And Solomon had 40,000 [sic] stalls of horses for his chariots, and 12,000 horsemen. (1 Ki. 4:26)
>
> Now Solomon had 4,000 stalls for horses and chariots and 12,000 horsemen, and he stationed them in the chariot cities and with the king in Jerusalem…

And they were bringing horses for Solomon from Egypt and from all countries. (2 Chron. 9:25,28)

And Solomon amassed chariots and horsemen. He had 1,400 chariots, and 12,000 horsemen, and he stationed them in the chariot cities and with the king at Jerusalem…
And Solomon's horses were imported from Egypt [Mizraim] and from Kue [Que]; the king's traders procured them from Kue for a price.
And they imported chariots from Egypt [Mizraim] for 600 shekels of silver apiece, and horses for 150 apiece, and by the same means they [the countries of Mizraim and Que] exported them to all the kings of the Hittites and the kings of Aram. (2 Chron. 1:14,16,17)

Also Solomon's import of horses was from Egypt (Mizraim) and Kue [Que], and the king's merchants procured them from Kue for a price
And a chariot was imported from Egypt (Mizraim) for 600 shekels of silver, and a horse for 150; and by the same means they exported them to all the kings of the Hittites and to the kings of Syria. (1 Ki. 10:28,29)

Now this is the account of the forced labor which king Solomon levied to build the house of the LORD, his own house, the Millo, the wall of Jerusalem, Hazor, Megiddo, and Gezer.
For Pharaoh king of Egypt had gone up and captured Gezer, and burned it with fire, and killed the Canaanites who lived in the city, and had given it as a dowry to his daughter, Solomon's wife.
So Solomon rebuilt Gezer, and the lower Beth-horon,
And Baalath and Tamar in the wilderness, in the land *of Judah*,
And all the storage cities which Solomon had, even the cities for his chariots and the cities for his horsemen … (1 Ki. 9:15-19)

All of the people who were left of the Hittites, the Amorites, the Perizzites, the Hivites, and the Jebusites, who were not of Israel,
namely, from their descendants who were left after them in the land whom the sons of Israel had not destroyed, them Solomon raised as forced laborers to this day.
But Solomon did not make slaves for his work from the sons of Israel; they were men of war, his chief captains, and commanders of his chariots and his horsemen. (2 Chron. 8:7-9)

From these texts we learn that Solomon's standing army consisted entirely of chariotry, with 12,000 trained horsemen, 4,000 horses and 1,400 chariots. There appears to be no foot soldiers in that standing army, though the final verses quoted above suggests that, if warfare were to come, the descendants of the ethnic Canaanites still remaining in Israel, employed in the duration as forced laborers in Solomon's extensive building enterprises, would be called into service. Meanwhile, native Israelites only were trained to serve as captains of this army, and native Israelites alone were involved in the chariot brigades. It was a formidable army, though throughout Solomon's extensive reign it was never deployed!

From the text of 1 Ki. 9:15-19, quoted above, we learn that the towns of Hazor, in the far north of Israel, Megiddo, near the Mediterranean coast west of the Jezreel Valley, and Gezer, on the coastal plain below Jerusalem, were the principal bases for the chariot army, as we might guess from the fact that they are singled out for re- construction in verse 15 (see

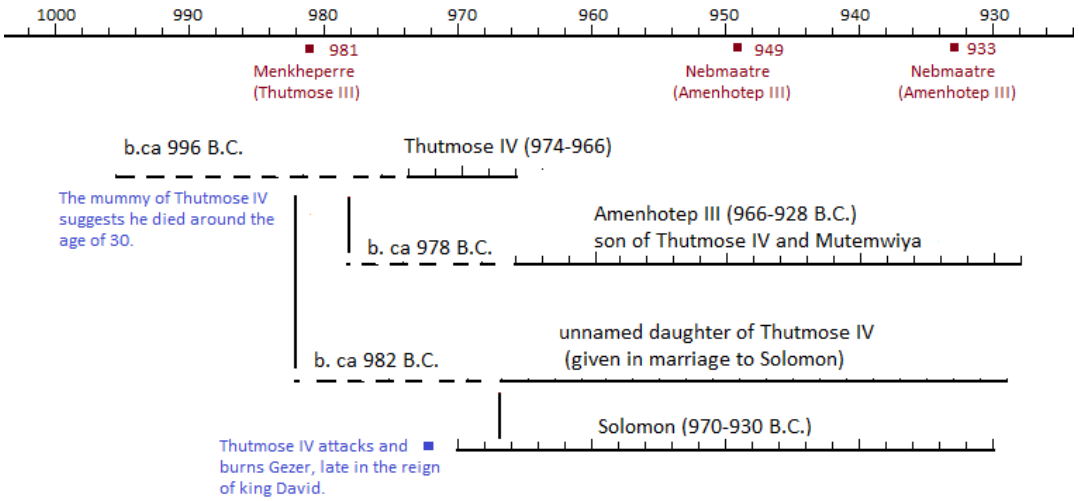
https://www.ministrymagazine.org/archive/1978/09/biblical-archeology) for archaeological evidence of this Solomonic construction in the three cities).

Finally, several verses in both the 1 Kings and 2 Chronicles passages emphasize that both the horses and the chariots were procured on the open market from Egypt (Heb. Mizraim) and Que/Kue (later known as Cilicia in the extreme SE of ancient Anatolia/present day Turkey). While there is no insurmountable problem in our interpreting the Hebrew text precisely as it is interpreted by most modern day scholars, we do take exception with one aspect of that interpretation. We do not believe that the Hebrew Mizraim in these few passages is a reference to Egypt, though we admit that this is the only word used in the Hebrew Bible to refer to Egypt. The reader needs to peruse pp. 84-89 in our volume 1, a section entitled *Tushamilki, king of Musru*. In that section we describe the utter stupidity of the actions described in an Assyrian text as interpreted thoughtlessly by modern scholars, who routinely interpret the Akkadian reference to a country called Musur/Musru as a reference to Egypt. While Musur/Musru is, in fact, the common designation of Egypt in Akkadian texts, there also existed at the time another country, with the identical name, situated in the extreme SE of ancient Anatolia, near the region of Que, that much more sensibly suited the circumstances described in the Akkadian text. And since the passage in question spoke of Musur and Que in the same breath, acting in concert in a military action, the term Musur must refer to that country. Failure to do that has resulted in a major distortion of Saite dynasty history. We would caution the reader of this essay not to let that happen again. Interpreting Mizraim here as a reference to Egypt would require us to consider Egypt as a manufacturing and distribution center for horses and chariots, supplying the Near East as far afield as the Hittite region of Anatolia and northern districts of modern day Syria. And that, in turn, would open up for criticism the text of 1 Ki 10:29 and 2 Chron. 1:17. And there is no shortage of critics willing to take up the task of discrediting the Hebrew Bible. The reader might be interested in reading the article written by the conservative Egyptologist K.A. Kitchen reviewing a book by Paul S. Ash, in which Ash has argued that the two references to Egypt in these two passages are mistaken. (See pages 43,44 of the review article by Kitchen entitled "Ancient Egypt and the Hebrew Monarchies" in the journal Themelios 26:3, pp 38-50, online at http://s3.amazonaws.com/tgc-documents/journal-issues/26.3_Kitchen.pdf . In this case, in our opinion, Kitchen is wrong and Ash is correct, but not because the Hebrew Bible is in error. In a moment we will confirm our opinion that in I Ki 10:29 and 2 Chron. 1:17 Mizraim should not be interpreted as a reference to Egypt.

Before we turn our attention to the situation in Egypt we should examine briefly the one text quoted above that we have not commented on. According to 1 Ki 9:16, at some unspecified time prior to the marriage of Solomon and an unnamed Pharaoh's daughter, this same Pharaoh had laid siege to Gezer on the coast of Israel, annihilating its troublesome Canaanite

population, and setting the city ablaze. The remains of that city were subsequently given to Solomon as (part of) the dowry of Pharaoh's daughter. There is need for us to construct a timeline of events, if for no other reason than to answer anticipated criticism of our existing timeline in Figure 1. The problem with that diagram, as the reader may already have noticed, is not in what it describes, but what it fails to explain. More specifically, it fails to explain what, precisely, is the relationship between Amenhotep III and the Pharaoh's daughter married by Solomon. The astute critic will notice that, if we were to add to that timeline additional information concerning the birth dates of the two kings Amenhotep III and Solomon, it would be obvious that the unnamed Pharoah in 1 Ki 9:16 cannot be Amenhotep III. As we have already noted, Egyptologists are agreed that Amenhotep III lived to be at most 50 years old, all but confirming the fact that he began his reign around the age of 12. If so, then he could hardly have fathered any offspring, much less a daughter old enough to marry king Solomon around the 3rd year of Solomon. This single observation confirms three facts, namely, 1) that the unnamed Pharaoh in 1 Ki 9:16 was Thutmose IV, the father of Amenhotep III, and 2) that Amenhotep III was therefore the brother-in-law, not the father-in-law of Solomon, and finally 3) that the destruction of Gezer was the work of Thutmose IV and therefore took place during that king's brief eight year reign, either in the latter years of king David or in the first year or two of Solomon's reign. We strongly favor the first option and argue that during the lengthy illness of king David, that which immediately preceded his death, Thutmose IV invested and burned Gezer. While we don't know why Thutmose took this action, we do not believe it had anything to do with an impending marriage. Returning the city to Israeli control and as part of his daughter's dowry, was probably an afterthought. Our Figure 2 below is provided to add some clarification to these formative years of the two kings in our earlier chart.

Figure 2: Timeline of the reign of Thutmose IV in relation to Amenhotep III and Solomon



Having dealt with the situation in Israel, we proceed with a description of the Egyptian army of Amenhotep III.

The Military of Amenhotep III

Considering the fact that Solomon and Amenhotep were roughly of the same age, were brothers-in-law, and clearly on amicable terms through the whole of their reigns, we expect that the military situation in Egypt will be virtually identical to that in Israel. And we are not disappointed.

The standing army of Amenhotep III consisted entirely of chariotry. There did exist a conscript army of foot soldiers, employed in the interim in various policing services, but as in Israel, these troops were apparently never utilized. As summarized by Fletcher (p. 106)

> Amenhotep III maintained his country's status as the super-power of its era through the civilized art of diplomacy rather than by force. The troops who were no longer needed for military campaigns were redeployed to guard borders and to police the trade routes and mining areas that created Egypt's great wealth. The latest scholarly research is revealing that the king did not neglect his armed forces.
>
> Amenhotep III seems to have been the first Egyptian king to employ chariotry as a separate section of the army: he used an elite chariot corps called the *maryannu* and appointed his father-in-law, Yuya, lieutenant-commander of chariotry. Horses were becoming more important in military engagements and the first Egyptian mounted soldiers were probably deployed around this time.

As for the foot soldiers mentioned by Fletcher, whose lives were lived out with virtually no military action (save for the few who participated in the solitary campaign beyond the southern border of Egypt in year 5), they consisted entirely of non-Egyptian recruits, largely from Nubia. According to Arielle Kosloff:

> Desert mercenaries were hired from Nubia, particularly from Wawat in the north and from the Medjay tribe. These were skilled and hardy fighters and were specifically requested by foreign rulers in the Amarna letters as *Melukha* – "dark-skinned" warriors – when in need of backup from Pharaoh. Within Egypt, particularly Upper Egypt, they served as something of a desert police force (p. 72 in *Amenhotep III, Egypt's Radiant Pharoah*, Cambridge University Press, 2012).

We should perhaps explain that all requests for Egyptian assistance in the Amarna letters date from the years of Akhenaton, when Egypt was once again militarily active north of the country.

We might conceivably end this segment of our discussion at this point, it being relatively clear that the Egyptian situation agrees favorably with what we expected. But as was the case with the description of Solomon's army, there is need to explain several items related to the horses and chariots being discussed, and certainly need to address the subject raised in our critique of

Solomon's chariot army, namely, whence did Egypt procure its horses and chariots and was it ever an exporter of these commodities? But before doing that, we need to briefly turn our attention to one comment further on in the section of Fletcher's book quoted earlier. The comment is instructive and we need to make note of it in passing. Though it does not relate specifically to our discussion of the army of Amenhotep III, it does help to clarify one troublesome aspect of our Figure 2.

In the course of pointing out that a number of the soldiers who served Amenhotep III were seasoned veterans, having served previous generations of Egyptian kings, Fletcher notes that at least one veteran soldier named Horemheb not only served under Amenhotep III, but also under Amenhotep's father and also his grandfather:

> "I followed the good god, lord of the two lands, Aakheperure [Amenhotep II], given life, his beloved son, lord of diadems, Menkheperure [Thutmose IV], given life, his beloved son, lord of the desert, Nebmaatra, son of Ra, Amenhotep, ruler of Thebes, beloved of Amun." (p. 107)

We mention this inscription simply because it accords well with the Figure 2 timeline, where we note that very little time is available in which to insert the 28 year reign of Amenhotep II, the father of Thutmose IV. The reader will note that only 6 years separate the conjectured ca 980 B.C. death of Menkheperre Thutmose III and the 974 B.C. beginning of the reign of his grandson Menkheperure Thutmose IV. Even when we argue, with Egyptologists, that the reign of Amenhotep II overlapped the last years of the reign of Menkheperre and that he also had a brief two year coregency with his son Menkheperure, the Berlin Stele allows him only a 10 year reign, roughly in the time slot 982-972 B.C. This brief reign is in general agreement with the statement of Horemheb. Because the reigns of the two 18th dynasty kings Amenhotep II and Thutmose IV take place in quick succession in our revised chronology, it is perfectly conceivable that Horemheb could have served under all three kings, even if he began his service early in the reign of Amenhotep II and was still serving in the middle of the reign of Amenhotep III. In the traditional history Amenhotep II ruled for 28 years, and some notable Egyptologists argue, based on the monuments, that Thutmose IV ruled for over 32 years. If so, then Horemheb is lying. How do we explain the situation?

The reader is reminded about the words which began this essay. Amenhotep III had no 25th dynasty namesake to confuse the interpretation of monuments which record his actions. Not so with Amenhotep II, nor for that matter with his son Thutmose IV. We have argued exhaustively on pages 152-181 of chapter 6 in our 2nd Volume (*Piankhi the Chameleon*) that most of the monuments which refer to kings by the names Aakheperure and Menkheperure belong to 7th century namesakes. This includes all of the monuments which furnish the high regnal year numbers for the two kings. If the 18th dynasty Aakheperure Amenhotep II ruled Egypt for only 10 years this is not a problem for the Displaced Dynasties chronology. It is a

devastating development for the traditional history, which is but one of the reasons Egyptologists are compelled to discount entirely the testimony of the Berlin Stele.

We leave this matter by quoting only the brief introductory material and related footnote from pages 169-170 of our Volume 2, in a section entitled *Menkheperure Thutmose IV*. Please note that we are not here arguing for a longer reign for the 18th dynasty king who bears that name, only that there did exist a king Menkheperure whose reign lasted that long. But this king lived in the 7th century.

> According to the traditional history the 18th dynasty king Amenhotep II was succeeded by a son Menkheperure Thutmose IV whose reign lasted either an abbreviated 8 years or an extended 32 plus years. The longer reign length is supported by such notable scholars as Wente and Van Sicelen, based primarily, though not exclusively, on monuments which suggest that the king celebrated at least one heb seb (30 year) festival.[1] In our opinion there is no getting around the evidence supporting the longer reign length. [Some king named] Menkheperure ruled at least 30 years, probably much longer. Those who argue for the minimum figure are clearly influenced by the anatomical reports related to the mummy of Menkheperure. We will examine those reports shortly.
>
> [1]Edward Wente and Charles Van Siclen III, "A Chronology of the New Kingdom," in *Studies in Honor of George R. Huighes* SAOC 39 (1977) 227-230. These scholars suggest a reign length of 33 years for Thutmose IV. In defense of this extended reign they state: "Besides the jubilee evidence in favor of a long reign for Thutmose IV there are some additional bits of information that would indicate that his reign was not a short one. There are statues of Thutmose IV seated, not with his queen as is normal, but with his mother Tiaa. At the time when he acceded to the throne, Thutmose IV was called an inpw, a term applied to young princes and kings who had not yet reached puberty. Yet by the time he died, this pharaoh had produced a rather large family, comprising at least seven sons and twelve daughters. During his years as king, Thutkmose IV had three queens, each of whom bore the title Great King's Wife. The Eighteenth Dynasty evidence seems to indicate that a king had only one Great King's Wife at a time, so that these three women must have held the title of principal queen consecutively. Another indication that Thutmose IV's reign was a long one is the large number of Theban tomb chapels that are assigned to his period. No fewer than nineteen tombs can be specifically dated to his reign ..." (*Piankhi the Chameleon*, pp 169-170)

Enough said concerning this matter at this time. The reader is encouraged to read the Volume 2, chapter 6, material referred to. As for the importation of horses and chariots to Egypt, and the possibity that Egypt became an exporter of such, we need quote only a sampling of scholarly opinion on the matter, beginning with one further remark from Arielle Kozloff's *Amenhotep III, Egypt's Radiant Pharaoh* , on the identical page quoted earlier.

> Chariot teams – the awesome war engines of antiquity – were a special division of the army. It is astonishing to think that horses and their obvious pendant, the chariot, were completely unknown in Egypt for the first 1,500 years or so of its written history, until the Hyksos invasion. Dynasty 18 pharaohs, however, quickly came up to speed in horsemanship, including breeding, husbandry, training, and driving. *By Amenhotep III's time, Egypt regularly imported fine horses from the Near East* and had also developed breeding programs of its own, which by then were somewhere around their thirtieth or fortieth generation of steeds. Kosloff, Amenhotep III: Egypt's Radiant Pharaoh, p. 72 (emphasis added)

Quotes such as this abound in the scholarly literature. Egyptologists are in general agreement that Egypt imported most of its horses, especially quality breeds, and it is counter-intuitive to

think that they had a "buy and sell" mentality and were also involved in the export trade. A similar situation prevailed with respect to chariots:

> While the obvious understanding of the water-proofing properties of birch bark and the use of elm in chariot making in Egypt indicate northern influences, the combination of native tamarisk with imported elm in the six-spoked chariot wheel of Amenhotep III suggests manufacture of chariots in Egypt itself – as vividly illustrated in workshop scenes in Theban New Kingdom tombs … On the other hand, comtemporary texts mention the import of timber from the Near East to Egypt, as well as the arrival of fully finished chariots as booty and a part of diplomatic exchange … When such chariots appear in the Egypt pictorial record, they are usually similar to Egyptian ones.
>
> Taking all the evidence into account, it is clear that the type of chariot used in Egypt is of Near Eastern origin. The transfer of chariot technology – and the keeping of horses – from the Near East to Egypt, not only included woods such as elm and birch (bark), but also skilled craftsmen, horses and their trainers, as well as the relevant technical vocabulary. (Joost Crouwel, pp. 84,85 in "*Studying the Six Chariots from the Tomb of Tutankhamon – an Update*" pp 73-94 in *Chasing Chariots: Proceedings of the First International Chariot Conference [Cairo 2012),* Andre J. Veldmeijer & Salima Ikram, eds., Sidestone Press, Leiden, 2013)

We need to qualify this quote only slightly. The workshop scenes referred to by Crouwel are likely only repair workshops. Chariots suffer extensive wear and tear, even in non-combat situations, and were certainly in constant need of repair. Importing the necessary woods and skilled craftsmen would of necessity be part of a routine maintenance program for this imported product. From a photo of an automobile service center today we would never conclude that this facility manufactured automobiles. Neither is there justification for concluding that the Egyptian workshops manufactured chariots. To my knowledge, there are no texts that make the claim that chariots were fabricated in Egypt. And there are certainly none that speak of the export of this product, even as gifts in the exercise of diplomacy in relations with other nations.

We leave the matter there. We have demonstrated a remarkable congruity between the nature and actions of the armies of Solomon and Amenhotep III.

One closing remark is in order. Since we are on the topic of chariots, we would be negligent if we failed to mention Egyptian chariots in their relation to the topic we have mentioned repeatedly, namely, the problem of distinguishing between monuments belonging to 18th dynasty kings and their 25th dynasty namesakes. The problem is restricted to the early kings of the 18th dynasty, from Amenhotep I through to Thutmose IV. What we have been unable to do is to suggest a foolproof means of determining to which dynasty we should ascribe monuments bearing the names of these kings. In fact, a possible solution to the problem exists, and it was suggested half a century ago by the Israeli archaeologist Yigael Yadin. And it involves the chariot, though, at the time, Yadin's suggestion had nothing to do with the dilemma we are talking about. A recent brief essay by James K. Hoffmeier has brought the matter to my attention. It needs to be investigated. We quote from Hoffmeier's journal article, entitled "*The Evolving Chariot Wheel in the Eighteenth Dynasty*", JARCE XIII (1976), p 43.

> It is generally believed that the horse-drawn chariot was introduced into Egypt by the Hyksos, and that it originated somewhere in the area of Syria-Palestine. Egyptian literary references to chariots occur as early as the reigns of Kamose, the 17th Dynasty king who took the first steps in freeing Egypt from the Hyksos, and Ahmose, the founder of the 18th Dynasty. Pictorial representations, however, do not appear until slightly later in the 18th Dynasty. Two of the earliest scenes are found in the tomb of Rennie of El Kab (possibly dating to the reign of Amenhotep I), and in the tomb of User (reign of Thutmose I). Professor Yigael Yadin maintains that during the earlier part of the 18th Dynasty, the Egyptian chariot was "exactly like the Canaanite chariot:" both were constructed of light flexible wood, with leather straps wrapped around the wood to strengthen it, and both utilized wheels with four spokes. In Yadin's eyes the four-spoked wheel is diagnostic for dating purposes; it is restricted to the early period of the 18th Dynasty. It remained in vogue, he says, until the reign of Thutmose IV, when "the Egyptian chariot begins to shake off its Canaanite influence and undergo considerable change." Yadin believes that the eight-spoked wheel, which is seen on the body of Thutmose IV's chariot, was an experiment by the Egyptian wheelwrights, who, when it proved unsuccessful, settled thereafter for the six-spoked wheel. In short, "**So widespread and meticulous is the delineation of the number of wheel spokes on chariots depicted on Egyptian monuments that they can be used as a criterion for determining whether the monument is earlier or later than 1400 B.C.**" [quoting Yigael Yadin, *The Art of Warfare in Biblical Lands*, I (Toronto, 1963), p. 87] (emphasis added)

Hoffmeier goes on to cite exceptions to Yadin's "rule of thumb", i.e. instances where dignitaries who served 18th dynasty kings, and in some cases the kings themselves, including Thutmose IV and most of his predecessors, are pictured on monuments which picture chariots with both four and six (or sometimes eight) spokes, thus appearing to discredit the methodology. But it is possible that the "exceptions" he cites actually prove that the method works, because the monuments for the kings he mentions possibly originate from both the 18th and 25th dynasty, some preceding and some following the 18th dynasty Thutmose IV. And if the methodology is sound, it might well serve to distinguish between monuments belonging to the 18th and those belonging to the 25th dynasty. The matter needs to be investigated further.
http://www.michaelsheiser.com/PaleoBabble/HoffmeierObservationsEvolvingChariotWheelinEgypt.pdf

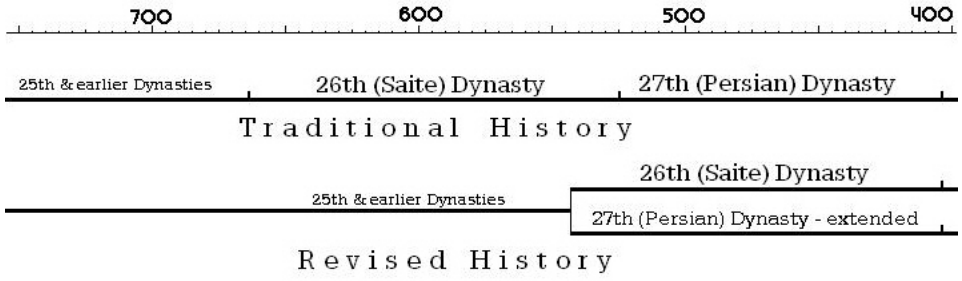
Paper #3 Revising the dates of the Phoenician kings of Byblos (ancient Gebal) serves to validate the 840-774 BC revised dates for Ramses II.

This paper will be longer than most in this new series and for good reason. In order to convince readers of the truth of any chronological facts it is essential that those readers have some familiarity with the timetable under consideration. So rather than leave it to individuals to digest the content of the 860 pages in the first three books of our *Displaced Dynasties Series*, we provide here a nine page summary of the history of the time frame under consideration, outlined from the point of view of the revised history. Needless to say, those individuals already conversant with our revised chronology can safely ignore these nine pages, though we have structured this summary to highlight material most relevant to our upcoming discussion of the Byblos kings. It is therefore recommended reading.

A. Overview of the Revised History of Dynastic Egypt in the years 774-650 BC

In the first book in our series we moved the 139-year-long 26th dynasty of Egypt forward 121 years from the time frame 664-525 BC to the years 543-404 BC, overlapping completely the 27th (Persian) dynasty, and extending the length of the latter to accommodate the move. Figure 1 below, borrowed from page 40 of our first book, visualizes the process.

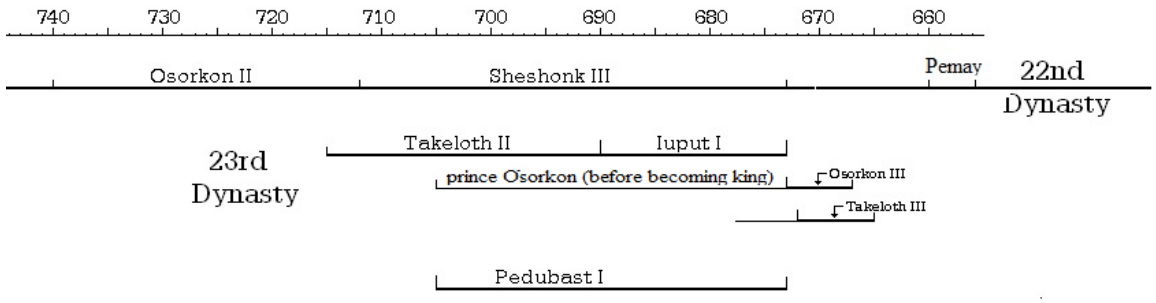
Figure 1: Timeline – The Saite Dynasty Displaced 121 Years



We then began the process of filling the vacated years with detail, it being the case that all dynasties earlier than the 26th must necessarily move forward in time by at least 121 years, a domino effect. For sundry reasons we decided to begin this process by demonstrating, in chapter three of book one, that the Egyptian 22nd and 23rd dynasty pharaohs, who in the traditional history occupy the

approximate time frames ca 945-730 BC and ca 817-730 BC respectively, must instead be dated in the interval between the mid-8th and mid-7th centuries BC. Our focus in book I was largely confined to the activities of the 22nd dynasty pharaohs Osorkon II (740-712), Sheshonk III (712-673), and Pemay (660-654) and their interactions with the 23rd dynasty kings Takeloth II (715-690), Pedubast I (705-679), Iuput I (691-673), Osorkon III (673-667) and Takeloth III (672-665), for whom we determined the regnal years indicated. For the time being we left out of consideration the earliest kings of the 22nd dynasty, Sheshonk I, Osorkon I, and Takeloth I. Our Figure 2 below, borrowed from page 75 of book 1, diagrams the results of our research.

Figure 2: Revised placements of some of the intermediate kings of Dynasties 22 & 23.



Following our book one chapter three research we returned to the subject matter which motivated this book one revision, and the balance of the first volume was spent authenticating our displacement of the 26th dynasty. But our brief foray into matters concerned with the 22nd and 23rd dynasties had brought to our attention at least five important facts related to the time frame under consideration in Figure 2, all of which have some bearing on our discussion later in this paper. We itemize them here.

1) Around the year 674 BC the Assyrian king Esarhaddon attacked Egypt and was rebuffed by the Egyptian pharaohs, his army driven from the country. Three years later, in 671 BC, he returned and successfully overran Egypt, establishing Assyrian suzerainty which lasted through the balance of his reign and that of his son Ashurbanipal, ending only when the 25th dynasty king Piankhi liberated Egypt around the year 637 BC. The annals of the Assyrian kings Esarhaddon and Ashurbanipal, those dated between the years 671 and 665 BC, inform us

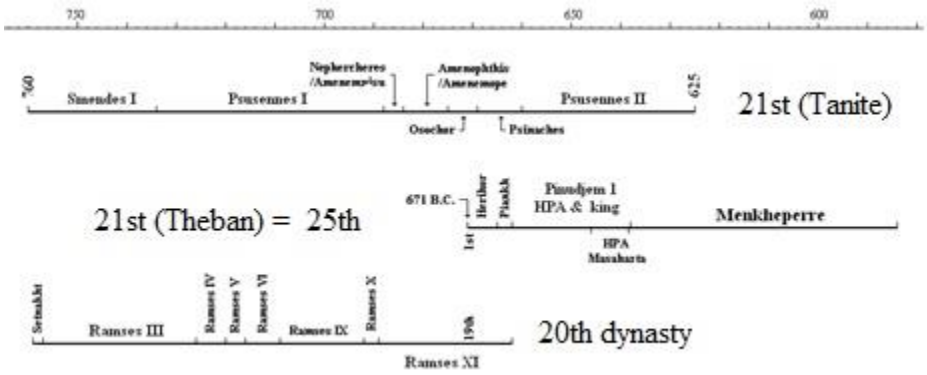
concerning the administrative structure of the country at that time. Among many important details they record the fact that the administration of the district of Busiris was left to a king named Sheshonk, clearly the 22nd dynasty pharaoh who belongs in the 13 year gap between Sheshonk III and Pemay in Figure 2, a space we left unfilled when writing our first book, not yet convinced of this king's identity. That identity crisis was addressed in our book two Appendix B. [Please note *that the link will take you to Appendix A, requiring you to scroll down to pages 294-301 where we discuss this pharaoh in some detail*] There we remark on the fact that the Egyptologist Aidan Dodson had in 1993 identified the king reigning between Sheshonk III and Pemay as yet another Sheshonk, and furthermore, had concluded that the prenomen of this Sheshonk was the identical Hedjkheperre Setepenre as that possessed by pharaoh Sheshonk I, who is identified (in the traditional history) as the founder of the dynasty. The 22nd dynasty was now blessed with two kings with the identical name, at least if we believe traditional historians. But that fact is called into question in our book two Appendix A (pages 284-293), where we detail what must certainly be the genealogy of both of these Sheshonks, and where we conclude that they were *probably* not namesakes. While we agree with Dodson that the pharaoh named Hedjkheperre Sheshonk did govern a portion of Egypt in the years 673-660 BC, between the reigns of Sheshonk III and Pemay, we are less convinced that the Sheshonk who founded the dynasty had the identical prenomen.

2) When we revised the dates for the dynasty 22 and dynasty 23 pharaohs in chapter three of book one, the relative chronology of those kings did not change appreciably from relative dating accepted by the traditional history. In most instances only the background changed along with the revised absolute dates, the intrusion of the Assyrians being the most conspicuous feature. According to the traditional history Osorkon II governed a portion of Egypt in the approximate years 872-837 BC. We assign him the dates 740-712 BC, a reduction of roughly 130 years, at least consistent with our 121 year lowering of dates for the 26th dynasty.

One feature only of the combined 22nd/23rd dynasty revised chronology is noteworthy here, namely, the fact that Egypt was not governed by a single pharaoh at any one time in the ca 760-650 time frame. It is Egyptologists, not the author of this paper, who first drew attention to the fact that Egypt was

extremely fragmented during the tenure of the 22nd and 23rd dynasty pharaohs, with multiple rulers (each identifying himself as pharaoh) governing different parts of Egypt concurrently, areas referred to in the history books as nomes (aka provinces). In fact, considering the multiplicity of kings that governed at any one time, it might be preferable if we refer to the 22nd/23rd dynasty monarchs as nomarchs, not pharaohs, and that fact became increasingly apparent as we continued with our book two research, where we quickly became convinced that many of the other nomes in Egypt were occupied by yet more claimants to the throne in that identical 750-650 BC time frame. In fact, as our research continued, we quickly became convinced that Egypt, throughout that approximate time frame, was governed by a multitude of kings belonging not to two, but to four concurrent dynasties, the 20th, 21st, 22nd and 23rd. The evidence was overwhelming. We leave it to the reader to peruse our book two revision for details (see in particular chapter 7, pages 182-214). Here we merely summarize the results of our research, and comment on a few salient features. Our Figure 3 below is borrowed from page 210 of book 2.

Figure 3 Timelines showing the positioning of the 20th, 21st (Tanite), and 21st (Theban) dynasties in the identical time frame occupied by the 22nd and 23rd dynasty pharaohs/nomarchs.



It is with the positioning of dynasties 20 and 21 that the enormity of the problem of misdated Egyptian dynasties becomes apparent. In the traditional history, following Manetho, scholars have mistakenly positioned the 20th and 21st (Tanite) dynasties sequentially between the tenures of the 19th and 22nd dynasty kings, assigning them the respective time frames 1184-1087 BC and 1087-945 BC.

Consequently, the errant dating of all Egyptian dynasties prior to the 20th now escalates dramatically. Whereas our revised history was compelled to lower the dates for the 22nd dynasty king Osorkon II by just 130 years, the dates for Smendes I, the assumed founder of the 21st Tanite dynasty, had to be lowered by three times that amount. This dynasty, as stated above, began in the year 1087 BC according to Egyptologists. We have assigned to Smendes the years 760-734 BC, a reduction of 327 years (1087 – 760). The lowering of dates for the 20th dynasty is even more dramatic, since we assign to Setnakht, the founder of that dynasty, the years 759-757 BC, thereby reducing his dates a resounding 425 years (1184-759). That 425 year error, of necessity, must at minimum apply to all dynasties preceding the 20th. It should not surprise the reader, therefore, that in our book three we have dated the 19th dynasty king Ramses II in the years 840-774 BC rather than the 1290-1224 BC dates assigned him by Sir Alan Gardiner in his classic *Egypt of the Pharaohs.* This astounding 450 year reduction in Ramses' dates is not science fiction. The revised dates are supported by volumes of evidence, including the Berlin stele we will be discussing momentarily.

One further error in the traditional history is evidenced by our Figure 3, and needs to be mentioned in passing before we move on to the contents of book three. The error relates to the group of kings designated by Egyptologists as the 21st (Theban) dynasty, the middle timeline in our Figure 3. In the infancy of Egyptian historical research, soon after the 18th century successful translation of the hieroglyphic script, this group of kings appeared out of nowhere in Egyptian documents, seemingly contemporary with the kings of the 21st Tanite dynasty. But Manetho appeared to know nothing about them. They were, quite literally, an enigma, especially so since Egyptologists had already determined that the 21st Tanite dynasty kings belonged to the 11th and 10th centuries BC, and were, followed sequentially by the 22nd dynasty kings.

Compounding the problem of identifying these Theban kings was the fact that early Egyptologists had mistakenly determined that they ruled in the south of Egypt at precisely the same time that the Tanite priest/kings governed in the north, hence the designation of both groups as belonging to the same 21st dynasty. That was an egregious error. It is true that according to inscriptional evidence a 21st dynasty Theban king by the name of Pinudjem, the first to bear this name, was definitely a contemporary of a 21st dynasty Tanite king by the

name Psusennes, but other evidence makes it certain that the synchronism was with Psusennes II, as in our Figure 3, and not with Psusennes I, as in the traditional history. Based on our interpretation of the relevant documents, the Theban 21st dynasty connection with the 21st Tanite dynasty disappears entirely, raising the following question. If the so-called 21st Theban dynasty is unrelated to the 21st Tanite dynasty, who are these Theban kings and why does Manetho fail to acknowledge them? To which we answer: *Manetho does recognize them*. When we move the 21st dynasty forward in time by approximately three hundred years, and thus the 21st dynasty Theban kings by approximately four hundred years, the latter turn out to be the progenitors of the line of kings Manetho identifies as his 25th (Ethiopian) dynasty. Though Manetho does not include in his listing of 25th dynasty kings any of the names cited on our 21st Theban time line, he does insist that the second king of his 25th dynasty was named Piankhi, and we spend the first five chapters of our second book proving that Piankhi's adopted Egyptian name was Menkheperre Thutmose, and arguing that the Menkheperre named in our Figure 3 timeline [31]must be identified as Piankhi. The fact that Menkheperre is an Egyptian name, while several of the other 21st Theban kings bore non-Egyptian (and arguably Ethiopian) names, supports our argument that the Menkheperre in our Figure 3 timeline is actually Piankhi. Further support is provided by the fact that Menkheperre's grandfather in our timeline was named Piankh.

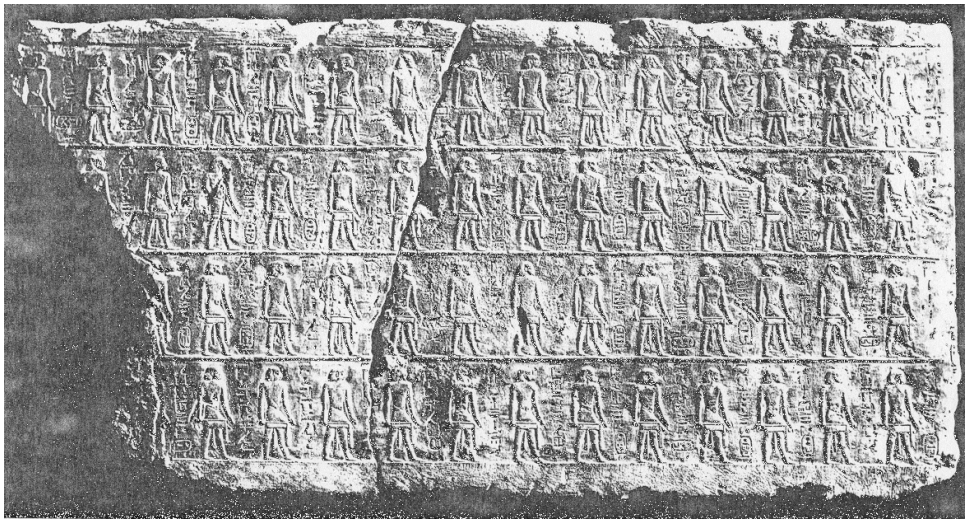
3) The third relevant discovery made early in our research, mentioned for the first time on pages 202-203 of volume two, was the existence of a massive stele, now housed in a Berlin Museum, that eventually served as our template for determining dates for dynasties 11-19. This stele (see Figure 4 below) contains a record of the succession of the high priests of the cult of Ptah in Memphis, based on documentation meticulously kept for over a thousand years of Egyptian history, spanning the approximate years 1500-600 BC.

As we state on page 202 of book two:

> In many instances the (Berlin stele) inscription names a king under whose rule a particular high priest held office. (For example), it states that two high priests ruled during the lengthy reign of Psusennes I, near the beginning of the 21st (Tanite) dynasty, while the high priest in the third generation prior ruled under Ramses II of the 19th dynasty. The 20th dynasty is noticeably absent from the document, leading to

speculation (among Egyptologists) that an haplography has caused the artisan to omit entirely the line of priests contemporary with the Ramesside kings of the 20th dynasty.

Figure 4 The Berlin Stele



Other anomalies in the document convinced Egyptologists that the Berlin stele inscription could not be relied upon to validate the existing chronology of dynastic Egypt. We agree entirely with that sentiment, though we argue that the fault lies with the existing chronology of dynastic Egypt, not with the Berlin stele chronology. Its omission of the 20th dynasty is proof of its accuracy, not evidence that its chronology is defective. When we first encountered this monument we had already determined that the 20th and 21st dynasties were contemporaries. The priests of Ptah in Memphis flourished in the general area of the north-eastern Delta, an area controlled by the Tanite kings. By contrast, the Ramesside kings were tenured well over three hundred miles to the south, in the vicinity of Thebes. Which of the two groups of kings should we expect the priests of Ptah to reference in their documentation?

We strongly advise the reader, new to our Displaced Dynasties chronology, to familiarize themselves with this important monument. Of particular relevance are the comments on pages 202-03 of book two and especially pages 8-18 of book three.

4) A fourth observation relates to the extremely brief interlude which separates the death of Ramses II in the year 774 BC and the onset of dynasties 20, 21, and 22 around the year 760 BC. While it was a surprise to this author to discover that the dates for Ramses II had to be lowered by 450 years, it was even more surprising to find that only fifteen years separate the death of this 19th dynasty pharaoh from the approximate beginning of dynasties 20, 21 and 22. But the evidence was overwhelming, and we had no choice but to accept it. And in a moment we will see validation of these numbers in our analysis of the Byblos kings. And it should not escape the notice of our readers, that once we validate the fact that the beginning of dynasties 20-22 follow the death of Ramses II by at most a few decades, we are not only verifying the accuracy our dating of Ramses II, but also authenticating the reliability of the Berlin stele chronology, on which almost the whole of the revised history depicted in our books three and four depends.

5) In our second point we commented on the extreme political fragmentation which existed in Egypt beginning around the year 760 BC, a situation which actually escalated in the country in the years that followed, lasting through most of the following century. While the reader can appreciate somewhat the extent of this fragmentation by simply superimposing the timelines in our Figures 2 & 3, that procedure fails to adequately portray the extent of the diversification of political influence that occurred. There were literally dozens of powerful dignitaries in control of cities and provinces throughout the country. One writer, quoting the Egyptologist Klaus Bauer, underscores the severity of this political fragmentation and distribution of power by commenting on diversification within the obscure 23rd dynasty alone:

> The 23rd dynasty has traditionally been viewed as a single line of kings beginning with Pedubast I, this following Manetho. Only recently has the argument been made that Takeloth II was a Theban pharaoh and that the 23rd dynasty begins with his reign. Klaus Bauer distinguishes five independent branches to this 23rd dynasty, with centers at Thebes, Tanis, Leontopolis, Hermopolis, and Heracleopolis ("The Libyan and Nubian Kings of Egypt: Notes," JNES 32 (1973) 4-25).

It is surely significant that when Esarhaddon and his son Ashurbanipal assumed control of Egypt in the years 671-637 BC, they left the governance of the country in the hands of approximately twenty nomarchs, several bearing royal titles. We presume that these individuals were not newly installed in their respective

districts. It is almost a certainty that the Assyrian kings merely left the existing nomarchs in place to administer the country on their behalf.

We repeat from our second point this claim of extreme political fragmentation within Egypt in order to raise the following question. What brought about this extreme diversification of political authority within Egypt in the span of approximately fifteen years, the time separating the death of the all-powerful pharaoh Ramses II (774 BC) and the situation where twenty or more nomarchs shared power within Egypt (760 BC)? In book three of our Egyptian series we provide the answer. Even the most casual student of Egyptian history knows that in the brief interval separating the reigns of Ramses II and Ramses III something monumental happened in the Mediterranean world. Some natural disaster brought to an end the Anatolian kingdom of the Empire Hittites, ended the Minoan civilization, and spawned massive migrations of desperate peoples occupying nations bordering the Mediterranean, some of them extremely militaristic - the so-called Sea Peoples of the history books. The fact that this natural disaster brought the 19th dynasty to an end, populated Egypt with foreigners, of which the Libyans (the assumed ethnicity of the 22nd and 23rd dynasty nomarchs) were the most prominent element, are facts agreed upon by all Egyptologists. Where the revised history diverges from the traditional history is in the dating of this natural disaster, and the fact we do not believe that the 20th dynasty pharaohs managed to drive the Libyan invaders out of Egypt.

In our book three we identified the source of this Sea Peoples movement - the eruption of the mega-volcanic island of Santorini, north of Cyprus. We will not repeat here our description of the event. The interested reader can follow our description beginning on pages 113-117 and continuing through the whole of chapter six (pages 118-140), the concluding chapter of our third book. Only one aspect of that catastrophic event bears repeating here. On pages 124-129, in a section entitled “Raash in Syria”, we documented the devastating results of the massive tidal waves and rainstorm of molten ash that fell on the east coast of the Mediterranean, the homeland of the Phoenician kings. Following the description of the event, in the book of Amos in the Hebrew Bible, we noted that 90% of the population of the Levant died as a result. What we did not discuss was the inevitable destruction of property that took place, including inevitable damage to

even megalithic structures such as the temples of the country, a fact which has some bearing on our dating of the Phoenician king named Yehimelek.

With that we turn our attention to Phoenicia, to the city state of Byblos (ancient Gebal) on the eastern coast of the Mediterranean, and to the kings of that maritime city.

B. Phoenician Inscriptions and Assyrian annals serve to validate the accuracy of our Revised Chronology, especially the 840-774 BC dates for Ramses II.

For the benefit of the uninformed, we begin this section by itemizing the most often cited inscriptions referred to under the rubric *Byblian Royal Inscriptions*, providing a brief description of the discovery and publication of each. We cite them here in the order in which they were discovered. They are five in number, but for completeness we add a sixth, perhaps the most valuable of all in validating our dating of the Egyptian 19th dynasty.

All of these inscriptions were written in a script known popularly as Old Byblian, though in reality the script was employed at locations throughout the Levant. If follows that it is best described as early Phoenician, a variant of paleo-Hebrew, the latter conceived by traditional scholars as a derivative of the former (though in fact, as we will demonstrate in later papers, the Hebrew script without question antedates the Phoenician).

1. **The Abibaal Inscription**, incised on a statue of a 22nd dynasty king named Hedjkheperre Sheshonq , published in 1903 by Charles Simon Clermont-Ganneau,

Inscription égypto-phénicienne de Byblos, Comptes rendu, Académie des inscriptions et belles-lettres (Paris, 1903). A translation of the inscription is provided here:

(1) [The statue (?) which] Abibaal, king of [Byblus, son of Yeḥimilk (?), (2) king] of Byblus, [br]ought from [42] Egypt for Baal[ath-Gebal, his lady. May Baalath-Gebal prolong the days of Abibaal and his years] over Byblus!

The above translation, and thus the claim that Abibaal was a son of a king named Yehimilk(?), is attributed to the noted Semitic scholar William Albright, who examined photographs of multiple Phoenician inscriptions in 1947 and published his results in an article entitled "The Phoenician Inscriptions from the 10th century B.C. from Byblus" (JAOS 67: 153-160). Since this pivotal paper will be cited several times in the pages that follow, but will be inaccessible to most readers, we have included a photocopy of his article at the end of this paper (see pages 24-31). But in addition to the translation above, we also include here a copy of Albright's table of Phoenician kings duplicated from his page 160, the last entry in the article. Due to Albright's standing in the academic community, these dates for the Phoenician kings have endured, relatively unchanged, through to the present.

Ahiram	c. 1000
Ittobaal (son of Ahiram)	c. 975
Yeḥimilk	c. 950
Abiba‘al (son of Yeḥimilk?)	c. 930
Elibaal (son of Yeḥimilk)	c. 920
Shipiṭ-Ba‘al I (son of Elibaal)	c. 900
. . . . .	
. . . . .	
Shipiṭ-Ba‘al II (Sipitti-bi‘il)	c. 740
Ormilk I (Uru-milki)	701
Milk-asap (Milki-ašapa)	c. 670
. . . . .	
. . . . .	
Shipiṭ-Ba‘al III [63]	c. 500 ?
. . . . .	
Ormilk II	
Yiḥar-Ba‘al (son of Ormilk II) [64]	
Yeḥaw-milk (son of Yiḥar-Ba‘al)	c. 450 ?
. . . . .	
El-pa‘al [65]	c. 360
‘Ozi-Ba‘al	348
Addir-milk	c. 340
‘Ayyin-El	333

The statue bearing the inscription of Abibaal bears the cartouche names Hedjkheperre Setepenre Sheshonk Meryamun, and is therefore credited by Egyptologists as belonging to the king by that name who founded the 22nd

dynasty in Egypt. In turn this Sheshonk is identified by traditional scholars as the pharaoh Shishak who assaulted the city of Jerusalem in the 5th year of Rehoboam, son of Solomon, in the approximate year 925 BC, a lynchpin date that supports the scholarly claim that this king ruled Egypt in the approximate years 943-922 BC, dates which mark the beginning of the Egyptian 22nd dynasty. Additionally, this king is claimed to be the author of the famous Bubastite Portal Inscription on a wall separating the 2nd portal of the Karnak temple in Thebes, and the small temple of Ramses III that opens into the first courtyard of that Karnak temple.

When Albright wrote his 1947 article Egyptologists were aware of only one Hedjkheperre Sheshonk. Now there are supposedly two, and in the revised history the second Hedjkheperre governed the Bubastite nome in Egypt around the years 673-660 BC (see above, pp 2-3). If so, it follows that he cannot be the Shishak who invaded Jerusalem 250 years earlier in the approximate year 925 BC. In a subsequent paper we will argue that the Bubastite Portal Inscription describes battles that took place around the year 674 BC. And since we believe that the Abibaal inscription was inscribed on a statue of this second Sheshonk, it follows that we should date the reign of Abibaal, king of Byblos, in the approximate time frame 680-670 BC, and the inscription itself to the year 674 BC. Time will tell if we are correct.

2. **The Ahiram Sarcophagus Inscription**, inscribed on a sarcophagus discovered in Byblos in 1923 in a tomb containing also two fragments of alabaster vases inscribed with the name of Ramses II, published by René Dussaud, *Les inscriptions phéniciennes du tombeau d'Ahiram, roi de Byblos*, Syria 5 (1924): 135–157.

Albright translates the sarcophogus inscription on pages 155-56 of his paper:

> The coffin which [22] [It]tobaal, son of Ahiram, king of Byblus, made for his father as his ab(o)de in eternity. And if [23] any king or any governor or any army commander attacks Byblus and exposes [24] this coffin, let his judicial scepter be broken, let his royal throne be overthrown,[25] and let peace [26] flee from Byblus; and as for him, let a vagabond(?) [27] efface [28] his inscription(s)!

For the most part we leave it to the reader to read the Wikipedia articles related to the discovery of the tomb of Ahiram, the reading of the inscription on the Ahiram sarcophagus left by his son & successor Ithbaal (or Ittobaal), and

especially the description of the other contents of the tomb, those which raised questions in the minds of scholars about the dating of the inscription. To simplify the stated task we choose to quote a few paragraphs from the book *Ramses II and His Time* authored by the noted revisionist Immanuel Velikovsky, who believes, as does the present author, that the Byblos king Ahiram must be a contemporary of the 19th dynasty king Ramses II.

> Near the entrance to the burial chamber several fragments of an alabaster vase were found, and one of them bore the name and royal nomen of Ramses II. Another fragment, also of alabaster, with Ramses II's cartouche was in the chamber; there was also an ivory plaque found and evaluated by R. Dussaud as of Mycenaean age; but pottery of Cyprian origin was also there and it looked like seventh-century ware. The tomb was violated, probably in antiquity, argued the historians, despite the warning in Hebrew (Phoenician) letters. The scholars had to decide on the time in which King Ahiram lived.
>
> The Phoenician inscriptions on the sarcophagus did not reveal it. Montet, the discoverer, assigned the tomb to the time of Ramses II, thus to the thirteenth century. He subscribed to the view that all objects in the tomb, the Cyprian vases included, were of the time of Ramses II. But the age of the Cyprian pottery was claimed by other scholars to be that of the seventh century. Dussaud, a leading French orientalist, agreed that the tomb dated from the thirteenth century, the time of Ramses II, but he insisted that the Cyprian ware was of the seventh century. Dussaud also assumed that in the seventh century tomb robbers broke in and left there the pottery of their own age. Signs of intrusion and violation were obvious: the lid of the sarcophagus had been moved from its proper position, alabaster vases were broken, jewelry was missing.
>
> Dussaud wrote: "Together with Mycenaean relics, Montet found fragments of Cypriote pottery, characteristic of the seventh century, which thus fixes the time of the tomb violation. No fragment of a more recent date was found." He continued: "There is no doubt that, [faced with a choice] between the age of Ramses II and the seventh century [as the time when the tomb was built and the inscriptions were made], the first must be accepted." **But intruders certainly would not have brought six- or seven-hundred-year-old vases into the sepulchral chamber. Why they would have brought any vessels into the mortuary chambers they had come to loot is not satisfactorily explained.** (emphasis added) (*Ramses II & His Time*, 65-66)

In our opinion Montet was absolutely correct. All the artifacts in the tomb are more than likely items actually owned by Ahiram, who must necessarily be dated to the time of Ramses II. But in the revised history Ramses II does not belong to the 13th century. His reign spanned the years 840-774 BC.

In our opinion Dussaud was also absolutely correct, save for his attribution of the Cypriote pottery to the tomb robbers. The Mycenaean plaque and the Cypriote pottery are not out of place, though we would dispute the seventh century date assigned the Cypriote ware. Pottery from that island was produced over the span of at least six hundred years, from ca 1000 to ca 400 BC according to the experts, though scholars continue to question the precise dating of the Geometric, Archaic and Classical production periods. There is no doubt, however, that the presence of Cypriote pottery in Ahiram's tomb does prove one thing, namely, that the 13th century traditional dates for Ramses II are at minimum three hundred years too early. As for the Mycenaean plaque, we need only remind the reader that when we move forward the dates for Ramses II by 450 years, we necessarily move forward the Mycenaean age by an equal amount. That age did not end until after the death of Ramses II. And when we claim, as we do in our revision, that the fall of Troy, and the probably survival of Agamemnon in Mycenae, must both be dated around the year 765 BC, those facts are absolutely consistent with our claim that Ahiram's life preceded that of Agamemnon by at least a half century, and possibly longer. His life was lived in a Mycenaean world.

In short, when we claim below that Ahiram's reign roughly spanned the years 820-800 BC, and those of his son Ithba'l the years 800-780 BC, we cannot be far wrong, a decade or so at the most. And with those dates absolutely every detail related to Ahiram's tomb is more than adequately explained.

3. **The Eliba'l Inscription**, inscribed on a statue of a 22nd dynasty king Osorkon and published by René Dussaud, *Dédicace d'une statue d'Osorkon 1er par Elibaal, roi de Byblos*, Syria 6 (1925): 101–117.

Albright translates the inscription on page 158 of his article:

> (1) ⌜The statue⌝ which Elibaal, king of Byblus, son of Yeḥi[milk, king of Byblus,] made (2) [for Ba]alath-Gebal, his lady. May Baalath[-Gebal] prolong (3) [the days of E]libaal and his years over [Byblus]!

It must be noted that the statue was inscribed with the cartouche names Sekhemkheperre-Setepenre Osorkon Meryamun. Egyptologists assign this name to their Osorkon I, the successor of their Sheshonk I, and we have absolutely no argument with the identification, other than to repeat our claim, made earlier in

this paper, that the Sheshonk who founded the 22nd dynasty probably did not bear the prenomen Hedjkheperre Setepenre.

We have always assumed that Manetho is probably correct in his claim that the 22nd dynasty began with three kings named Sheshonk, Osorkon and Takelot, all of whom must have occupied Egypt in the time frame ca 760-740 BC, this because we are adamant that the fourth king, Osorkon II, governed in the time frame 740-712 BC and that the 22nd dynasty began in Egypt around the year 760 BC. We suspect that one or more of the three predecessors of Osorkon II began their reigns before they invaded Egypt as part of the Sea Peoples movement. We suspect also that two or all three of these kings governed different parts of Egypt as contemporaries after entering Egypt. Egyptologists credit Osorkon I with either 33 years (Kitchen TIP p. 182 sect 150) or 36 years (Gardiner EP page 448) but Kitchen admits that the year 33, read on a Ramesseum bandage fragment, might actually read year 13, thus crediting Osorkon I with a much shorter reign, one more in agreement with Manetho who assigns this Osorkon 15 years.

Regardless, we can safely position the reign of this king somewhere in the time frame 760-740 BC, and date the inscription of Elibaal accordingly around the middle of that time frame.

4. **The Yehimelek Inscription**, published in 1930 by Maurice Dunand, "Nouvelle Inscription Phénicienne Archaique, RB 39 (1930): 321-331.

According to Albright (page 157) the inscription of Yehimelek, king of Biblos, reads:

> (1) The temple which Yeḥimilk, king of Byblus, built—(2) it was he who restored [33] the ruins of these temples. (3) May Baal-shamem [34] and Baal-(ath)-Gebal (4) and the assembly [35] of the holy gods of Byblus (5) prolong the days of Yeḥimilk and his years (6) over Byblus as a rightful king and a true (?) king [36] before the h[oly] gods of Byblus! [37]

We will say more about king Yehimelek in our next point, where he is identified as the grandfather of the Byblos king Shipitbaal. Our only concern at this time is to assign to him approximate dates, and since we agree entirely with scholarly community in positioning this king the successor to Ithbaal, son of Ahiram, to whom have assigned the dates 800-780 BC, we are inclined to position Yehimelek

the time frame 780-760 BC. Even if we were to disregard the consensus opinion of Egyptologists we would have selected this time frame for two reasons. **The first** relates to the successors of this king, three of whom are conclusively dated in the late 8th century BC. Already we have assigned to Elibaal the dates 760-740 BC. The others will be discussed in point 5 which follows. **The second** factor relates to a comment in the inscription itself, seldom if ever commented on by scholars. We refer to Yehimelek's statement that he was preoccupied with restoring "the ruins of these temples" presumably a reference to the temples in the vicinity of Byblos. And in the opinion of this author, there is only one possible reason why multiple temples in the district of Byblos should all be in ruins simultaneously, especially around the middle of the 8th century BC on the eastern coast of the Mediterranean.

We assume that Yehimelek was the king of Byblos at the time of the "great raash" recorded in book of Amos in the Hebrew Bible. There is no need here to repeat the comments on the subject made earlier in this paper. When massive tidal waves swept away and killed 90% of the population of the Levant in the approximate year 765 BC, clearly Yehimelek was one of the few survivors, and one of the driving forces in the restoration of the country which followed. Even temples made of stone would have suffered considerable damage. And temples would have been a top priority in the restoration effort. We assume therefore that the Yehimelek inscription was made around the year 760 BC, near the end of his life.

Before we proceed we summarize the conclusions made thus far. The table below reproduces the admittedly crude date ranges for the reigns of the five kings thus far discussed:

Table 1: Five 9th-7th century kings of Byblos

King of Byblos	Approximate regnal years (BC)	Approximate date of source (BC)
Ahiram	820-800	n/a
Ithbaal	800-780	800 (inscription)
Yehimelek	780-760	760 (inscription)
Elibaal	760-740	750 (inscription)
Abibaal	680-670	674 (inscription)

5. **The Shipitbaal Wall Inscription**, published in 1945 by Maurice Dunand, Biblia Grammata: *Documents et Recherches sur le Dévelopment de L'écriture en Phénicie (Beyrouth*: Direction des Antiquité, 1945): 146–151.

Once again we observe how Albright translates the inscription, this time on page 158 of his article:

(1) The wall which Shipiṭ-Ba'al, king of (2) Byblus, son of Elibaal, king of Byblus, (3) son of Yeḥimilk, king of Byblus, built for Baalath- (4) Gebal, his lady. May Baalath-Gebal prolong (5) the days of Shipiṭ-Ba'al and his years over Byblus!

Since this inscription clearly identifies Shipit-baal as a son of Elibaal and grandson of Yehimilk, we can, without further adieu, assign to him the date range 740-720 BC. And for the first time we can unequivocally confirm absolute dating of this time frame, not only from the point of view of the revised history, but from the annals of the Assyrian king Tiglath-Pileser III, whose approximate dates 745-727 BC are agreed upon by both traditional scholars and the Displaced Dynasty history.

Early in the reign of this powerful Assyrian king, sometime in first decade of his reign, Tiglath-Pileser raided the Phoenician coast, and recorded his conquests in his annals. The relevant section is translated by Oppenheim in Pritchard's classic collection of Ancient Near Eastern Texts (aka ANET), where we read:

[] I received tribute from Kuštašpi of Commagene, Rezon of Damascus, Menahem of Samaria, Hiram of Tyre, **Sibitti-bi'li of Byblos**, Urikki of Qu'e, Pisiris of Karchemiš, I'nil of Hamath, Panammu of Sam'al, Tarḫulara of Gurgum, Sulumal of Melitene, Dadili of Kaska, Uassarme of Tabal, Ušhitti of Tuhana, Tuhamma of Ištunda, Urimme of Hubišna, and Zabibe, the queen of Arabi - gold, silver, tin, iron, elephant-hides, ivory, linen garments with multicolored trimmings, blue-dyed wool, purple-dyed wool, ebony-wood, boxwood-wood, whatever was precious enough for a royal treasure; also lambs whose stretched hides were dyed purple, wild birds whose spread-out wings were dyed blue, furthermore horses, mules, large and small cattle, male dromedaries, female dromedaries with their foals. ANET 283 (emphasis added)

Menahem reigned in Israel in the latter half of the 8th century BC. According to the Wikipedia article describing his reign his dates vary, from 745-736 according

to Schrader, 745-738 in the opinion of Albright, and in the extreme, 752-742 according to Thiele in his Mysterious Numbers of the Hebrew Kings. Since the campaign referenced in ANET 283 probably took place around the year 740 BC, and Menahem is governing in Samaria, we must either extend Thiele's date range by several years, or accept the opinion of Albright and Schrader. In either case it is clear that these annals of Tiglath-Pileser suggest that we are near the beginning of Shipitbaal's reign, and can tentatively assign him the date range 740-720 BC, and lacking further information, date his wall inscription to the middle of that range, as does Albright.

Our table 2 now reflects this addition:

Table 2: Six 8th/7th century kings of Byblos

King of Byblos	Approximate regnal years BC	Approximate inscription date BC
Ahiram	820-800	n/a
Ithbaal	800-780	800 (inscription)
Yehimelek	780-760	760 (inscription)
Elibaal	760-740	750 (inscription)
Shipitbaal	740-720	740 (annals) 730 (inscription)
Abibaal	680-670	674 (inscription)

The addition of Shipitbaal does not prove that we have correctly dated the six listed kings, but it does strengthen our argument. Scholars are well aware of Tiglath-Pileser's annals entry, and the fact a Byblian king by this name did exist in the late 8th century. They simply refer to him as Shipitbaal II and they continue to argue that a Shipitbaal I, son of Elibaal, son of Yehimelek lived in the 10th century BC. And they strenuously argue that Shipitbaal I and Elibaal were contemporaries of the 22nd dynasty kings, whom they continue to date in the years 945-730 BC, as we mentioned earlier. But this assumption of namesake kings must be seriously questioned. As always, let the reader decide. Is it mere chance that this 8th century Phoenician king appears on the scene at precisely the right moment in time to fill the vacant spot in our Table 2? After all, we had already reasoned the existence of Phoenician kings Yehimelek and Elibaal in the 3rd and 4th positions of

our table 1. The next entry simply had to be a Phoenician Biblian king named Shipitbaal. And this will not be the first coincidental appearance of a Phoenician king at an opportune time, as we will see in our next inscription.

6. **The Yahawmelek inscription** is recorded on a large stele discovered near

Byblos around the year 1874 (see photo to the left). The inscription was first presented to the world in a communication made to the *Academie des Inscriptions et Belles Lettres* in the year 1875 by Melchior marquis de Vogüé. We have provided a link to this communication, which bears the title **Stele de Yehawmelek, roi de Gebal,** though regrettably, for the sake of our readers not conversant with French, we cannot find a suitable online translation of the French text of either the entire communication or the inscription contained in it. Regrettably also, Albright neglects to even mention the stele, though he is well aware of its content. Fortunately, this omission is not a problem, since we are concerned in this paper with only the initial line of the inscription, which reads:

> Je suis Yehawmelek, roi de Gebal, fils de Yahdibaal, petit-fils de Urimelek, roi de Gebal, race royale qne la dame Baalath-Gebal a établie sur Gebal.

Here Yahawmelek clearly identifies himself as a king of Gebal (ancient Byblos), son of Yahdibaal, and grand-son of Urimelek, king of Gebal. This inscription is of profound interest to this author, in spite of the fact that Albright omits mention of it in his article. Albright's avoidance is best explained by the late 6th/ 5th century dates he assigns to these three names (see his list of Phoenician kings duplicated on page 11 above). Other scholars date Yahawmelek a century or two earlier. Nothing in the inscription precludes our redating it to the early 7th century.

Though the inscription clearly describes the lineage of Yahawmelek, providing the names of his father Yahdibaal and grandfather Urimelek, the opening line recording that patrimony on the stele is clearly out of the ordinary, both in the omission of the title "king of Byblos" applied to Yahdibaal, and the reference to Urimelek as the grandfather of Yahawmelek. Those features have raised serious questions in the minds of all scholars commenting on the stele inscription. Here

we cite one instance only, that of Charles Clemont-Ganneau, on page 8 of an article entitled "La Stele de Byblos" published in 1880 in a fascicle of the Bibliotheque de l'Ecole Des Hautes Etudes.

Clemont-Ganneau remarks on the wording of the first line of the stele, particularly on the exceptional omission of the descriptive phrase "roi de Byblos" in relation to Yahdibaal and the inclusion of the Phoenician equivalent of the term grand-son. What would normally be expected in this type of genealogical reference would be a statement reading "Yehawmelek, king of Byblos, son of Yahdibaal, king of Byblos, son of Urimelek, king of Byblos. The language used on the stele appears to be deliberate, and Clemont-Ganneau draws from it the only reasonable conclusion, namely, that Yahdibaal, the parent of Yehawmelek, was not a king of Byblos, and that the governance of Byblos had passed directly from grandfather to grandson, a fact we assume to be the case in our table 3. In the words of Clemont-Ganneau:

> L'on comprend à la rigueur que le roi de Byblos Yehawmelek, fils de Yahdibaal, se rattache directement à son grand-père Ourimelek, roi de Gebal, en se disant בנבן, *petit-fils*, de ce dernier, son père Yahdibaal n'ayant pas régné et le mot בנבן exprimant alors, pour ainsi dire, la continuité de la royauté qu'on ne saurait concevoir comme interrompue, ne fût ce qu'un moment. Il est impossible de deviner pour quel motif Yahdibaal n'a pas occupé le trône : peut-être quelque évènement politique était-il intervenu pour l'en priver; peut-être était-il mort avant son père Ourimelek, si tant est qu'il fût le fils d'Ourimelek, ce dont on pourrait douter en se plaçant à un point de vue que j'indiquerai plus bas.

Of Clemont-Ganneau's suggested causes of this unusual succession, we accept as most probable his suggestion that Yahdibaal died before his father Urimelek, and that when Urimelek died the throne of Byblos passed directly to a grandson. That interpretation of the genealogical statement is reflected in the fact that we include only two names, rather than all three, in our Table 3 list of Phoenician kings. Our table also suggests that the Yahawmelek stele was likely erected to commemorate his ascendancy to the throne of Byblos, and likely follows immediately the death of Urimelek.

Table 3: Eight 9th-7th century kings of Byblos

King of Byblos	Approximate regnal years (BC)	Approximate date of source (BC)
Ahiram	820-800	n/a
Ithbaal	800-780	800 (inscription)
Yehimelek	780-760	760 (inscription)
Elibaal	760-740	750 (inscription)
Shipitbaal	740-720	740 (annals) 730 (inscription)
Urimelek	720-700	701 (annals)
Yehawmelek	700- 680	700 (inscription)
Abibaal	680-670	674 (inscription)

We assure the reader that we are not guessing when we position of the names of Urimelek and Yehawmelek as we do, between the names of Shipitbaal and Abibaal. The positioning is confirmed at both the upper and lower extremes.

On the one hand we are absolutely certain that a Phoenician king named Urimelek sat on the throne of Byblos in the last decade of the 8th century, and that this king was very likely the immediate successor of the Shipitbaal named in the annals of Tiglath-Pileser III. A glance back at Albright's list of Phoenician kings on our page 11 confirms that fact, though once again the traditional history is compelled to hypothesize the existence of namesake kings. In Albright's list the late 8th century Urimelek is referenced as Urimelek II, while the 10th century king, listed as a successor of Shipitbaal, is identified as Urimelek I. The source document for the late 8th century Urimelek is once again the annals of an Assyrian king, this time Sennacherib (705-681 BC), in his 3rd campaign, which included an assault on Judah and Jerusalem. The year was 701 BC., and the reader can read the relevant section of his annals both here and here).

On the other hand we are certain that Yehawmelek immediately preceded Abibaal. Recall from our earlier depiction of the Abibaal inscription that Albright inserted a question mark following his mention of the Phoenician king's father Yehimelek. Small wonder, since in his transcription of the Phoenician characters into Hebrew, he lists the phrase "son of Yehimelek" as . We have no

problem with the assumption that the "bet" at the start of the word is shorthand for "son of", but the five remaining characters suggest that the "yod", the third character in the name **Ye**(i)**m**e**l**e**k**, is not visible, or not identifiable. If that third character was a "waw", as we believe, the name should be read Yehawmelek, rather than Yehimelek. Enough said.

7. **Milki-ashapa, king of Byblos**. This king might well be omitted from this listing, save for the fact that Albright has included his name in his listing of kings of Byblos, assigning him the date c. 670 BC (see page 11 above). Since the source of this name is entirely consistent with our table 3 timetable, we simply follow Albright's lead.

We have previously described the fact that the Assyrian king Esarhaddon (681-669 BC) invaded Egypt twice, the second time (671 BC) successfully. The assault apparently was carefully planned and carried out, and lasted for well over a year. Today it is referred to as Esarhaddon's Syro-Palestinian campaign, during the course of which he recruited the assistance of multiple Phoenician kings, including that of Milkiashapa of Gebal (see ANET 291). Albright dates the annals entry to the year 670 (see page 11 above). Following the successful conquest of Egypt Esarhaddon returned to Nineveh where he died the following year. Again rebellion broke out in Egypt, prompting his son & successor Ashurbanipal to initiate his 1st campaign in order to quell the uprising. While en-route to Egypt in ca 668 the Assyrians again recruited the assistance of several dozen Levantine kings, among whom, again, we find reference to Milkiashapa of Gebel (see ANET 294). The texts of the annals which document these campaigns can be read by following the link provided to an online copy of Pritchard's Ancient Near Eastern Texts and scrolling down to the respective pages.]

Our listing of the kings of Gebal in the late 9th through early 7th centuries is complete, fixed in place by artifacts and historical circumstances, not to mention well dated entries in the annals of the Assyrian kings Tiglath Pileser III, Sennacherib, Esarhaddon and Ashurbanipal. We produce the completed list in our Table 4 on the following page. What makes this list compelling is not the individual entries viewed singularly, but the cumulative weight of the successive entries. The list of the kings of Gebel as presented clearly do not "prove" that Ramses II lived in the years 840-774 BC. Were that the case Egyptologists would long ago have cast aside the currently accepted chronology for dynastic Egypt.

Table 4: Nine 9th-7th century kings of Byblos

King of Byblos	Approximate regnal years BC	Approximate date of source BC
Ahiram	820-800	n/a
Ithbaal	800-780	800 (inscription)
Yehimelek	780-760	760 (inscription)
Elibaal	760-740	750 (inscription)
Shipitbaal	740-720	740 (annals) 730 (inscription)
Urimelek	720-700	701 (annals)
Yehawmelek	700- 680	700 (inscription)
Abibaal	680-670	674 (inscription)
Milki-ashapa	670-660	670 (annals) 668 (annals)

As we have seen, none of the inscriptions or annals entries referenced here are unknown to the current generation of scholars, and ad hoc explanations have been concocted to explain every anomaly. Rather, it is the accumulated weight of the evidence that must convince the reader that we are correct. Our dating of Ramses II was based on the combined evidence presented in 850 pages of carefully reasoned text in three books of our Displaced Dynasty Series, supported by the testimony of the priests of Ptah based on documentation preserved over a thousand years of temple activity, and literally carefully "written in stone". What we have added here is the supplementary witness of six late 9th- early7th century Geblite kings and four well known and securely dated Assyrian kings.

As always, let the reader decide.

The Phoenician Inscriptions of the Tenth Century B. C. from Byblus

W. F. Albright
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THE PHOENICIAN INSCRIPTIONS OF THE TENTH CENTURY B. C. FROM BYBLUS

W. F. ALBRIGHT
THE JOHNS HOPKINS UNIVERSITY

THE DISCOVERY in 1923 of the sarcophagus of Ahiram, king of Byblus, was at the time a scholarly sensation.[1] Two fragments of alabaster vases, found in the débris which filled the tomb itself and the adjacent shaft leading to the surface, bore the name of Ramesses II, and were naturally believed to date the contents of the tomb to the thirteenth century B. C. Moreover, several of the Phoenician letters in the Ahiram inscription were so archaic in appearance that it seemed only natural to date them centuries before the Mesha Stone, at that time the oldest datable document in the North-Semitic alphabet. Since the Mesha Stone is fixed by Israelite synchronisms to about 835 B. C., a date for the Ahiram sarcophagus in the thirteenth century did not seem unreasonable.

However, doubts began to be heard almost at once. When Dussaud issued the official publication of the Ahiram inscription in 1924 he included a discussion of the epigraph of Abibaal, king of Byblus, which had been published by Clermont-Ganneau in 1903.[2] Before 1924 no scholar had been able to decipher the text satisfactorily, owing to the presence of an archaic *kaph*, which was then read as *shin* and dated much too late. The inscription had been incised on a large fragment of a statue of Shishak, first king of the Twenty-second Dynasty, who reigned cir. 935-915 B. C.[3] It now became clear that the Phoenician text must have been incised on the statue during the lifetime of Shishak, or immediately after the latter's death. The script of the Ahiram and Abibaal inscriptions is so nearly identical that it no longer seemed quite so evident that the former dated at least three centuries before the latter.

In 1925 Dussaud published a torso belonging to a statue of Osorkon I (cir. 915-885 B. C.), son and successor of Shishak, three fragments of which had been known for several decades and had finally been acquired by the Louvre.[4] Additional fragments of the same statue were excavated by Montet and Dunand at Byblus in the following years.[5] This statue bore an inscription of Elibaal of Byblus, in the same archaic script, obviously almost contemporary with the statue itself. Dussaud's publication was followed by the discovery of other inscriptions in the same script, especially that of Yeḥimilk, king of Byblus, published by Dunand in 1930.[6] None of these other inscriptions added anything to the solution of the chronological picture.

The present writer had meanwhile been lowering his date for Ahiram from the twelfth century to

[1] On this inscription see the bibliography given by Montet, *Byblos et l'Égypte* (1928), p. 238, n. 1, to which should be added especially Torrey, *JAOS* 45 (1925), 269 ff.; S. A. Cook, *Pal. Expl. Fund, Quar. State.*, 57 (1925), 210 ff.; Albright, *JPOS* 6 (1926), 76 ff.; S. Ronzevalle, *Mélanges de l'Université St.-Joseph*, 12 (1927), 1-40; Lidzbarski, *Or. Lit.-zeit.*, 1927, cols. 453 ff. Reproductions of the text, with photographs, are given by Montet; Vincent, *Rev. Bib.*, 1925, 183 ff. and Pl. VIII; Ronzevalle, *op. cit.*, Pl. I-IV, who gives by far the best.

[2] See *Syria*, V (1924), 145 ff., and for a bibliography of the inscription see Montet, *Byblos et l'Égypte*, p. 57; the latest treatment is by B. Maisler, *Leshonenu* 14 (1946), 174 f. For photographs see Clermont-Ganneau, *Recueil d'Archéologie Orientale*, VI, 74 and Pl. II.

[3] See *BASOR*, No. 100, 16 ff., for my dating of Rehoboam (cir. 922-915), on which the chronology of Shishak must rest, since the fifth year of the former fell not long before the 21st of the latter: i. e., Shishak's 21st year must be dated soon after 918/7 B. C.

[4] See Montet, *Byblos et l'Égypte*, p. 54, for a bibliography; the text is published by Montet on Pl. XXXVI-XXXVII. Additional fragments of the statue were published by Dunand, *Fouilles de Byblos*, I (1939), 17 f., one containing a vital word of four letters.

[5] Three fragments of the statue (one of which seems later to have been lost) were reported in 1881 in a private collection and two subsequently entered the Louvre; three were excavated by Montet and two were recovered by Dunand in the neighborhood.

[6] See *Rev. Bib.*, 1930, 321 ff., and the official edition in *Fouilles de Byblos*, I, 30 and Pl. XXXI.

about 1000 B. C. ("not later than cir. 975 B. C."), against strong opposition from most scholars.[7] However, the discovery of alphabetic inscriptions from the thirteenth century B. C. at Lachish and elsewhere in Palestine made it increasingly clear that the script of these Byblian documents was much too developed to belong to that century. In 1942 the writer pointed out that the potsherds of Cypriote type which were found in the débris of the entrance shaft could not antedate the middle of the eleventh century.[8] In 1943 Aimé-Giron independently dated the Ahiram sarcophagus in the tenth century,[9] and in 1946 Maisler followed him in this reduction of the chronology.[10] Finally, in April, 1946, Dunand reversed his earlier position after a detailed study of the sherds from the entrance shaft, and dated the sarcophagus about 1000 B. C.[11] There is, accordingly, at long last, general accord as to the date of this important monument.

In 1945 Dunand published a new royal inscription from Byblus, belonging this time to a king named Shipiṭ-Ba'al, whom he dated provisionally in the sixteenth or seventeenth century B. C.[12] Since his reduction of the date of Ahiram by a quarter millennium, he would perhaps also lower this date by several centuries. Aside from the *beth*, all letters in this and a shorter inscription published at the same time have substantially the same form as in the other, previously published, royal inscriptions from Byblus. In 1946 both Maisler and the writer came out against Dunand's relative chronology, placing Shipiṭ-Ba'al after the tenth-century group already established, not before it.[13] The forms of letters are in a number of cases relatively late and the aberrant *beth* is probably an ephemeral cursive form, without influence on the development of the script as a whole.[14] Maisler was inclined to lower the dates of these rulers either to the period 950-750 B. C. or to 1000-800 B. C.[15] In the writer's judgment there is no need to date any of them after the beginning of the ninth century, and the group as a whole belongs to the tenth century; see below for details.

When the first documents of this category were published there was much less external evidence bearing on grammar, lexicography and spelling

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[7] See "The Role of the Canaanites in the History of Civilization," *Studies in the History of Culture* (*Leland Anniversary Volume*), 1942, pp. 34 f., n. 78, and the epigraphic discussion, *BASOR*, No. 92 (1943), 19. Spiegelberg and Lidzbarski had never accepted the high chronology of the excavators.

[8] Cf. the preceding note. This argument holds, but it must be remembered that the middle of the eleventh century is a *terminus a quo*; the sherds may easily be half a century or even a century later. It must also be remembered that there seems to be something of a lag between the appearance of this pottery in Phoenicia (as controlled by Palestinian evidence) and its spread to Cyprus.

[9] *Annales du Service des Antiquités de l'Égypte*, 1943, pp. 284 ff., quoted from Maisler, *op. cit.*, p. 178, n. 53.

[10] *Op. cit.*, p. 178.

[11] *Byblia Grammata*, "Post-scriptum," pp. 197 ff. (circulated after publication of the book). His revision of previous views is based primarily on the finding of typical Iron-I pottery in some unexcavated débris in the shaft of the Ahiram tomb. His further view that the sarcophagus was usurped by Ittobaal for his father and that the carved representations on it are older than the latter is very tempting, though not yet demonstrable because of our lack of comparable Phoenician reliefs of known date.

[12] *Byblia Grammata*, Beyrouth, 1945, pp. 146 ff. The high date is based primarily on the context in which the 'Abda' sherd was discovered (p. 155), but since the sherd was not sealed in with these objects by a floor or layer of ashes and since the objects themselves do not seem to be very characteristic, the argument remains weak. To me the 'Abda' sherd looks like an object from Early Iron I (cf. also Maisler, *op. cit.*, pp. 167 f., though he inclines to a still later date, for which there is no evidence, in my opinion).

[13] See Maisler, *op. cit.* (*Leshonenu*, 14, 166 ff.); the writer, *BASOR*, No. 102, 20, and 103, 14 f.

[14] It is now certain that the peculiar quasi-cursive *alepḥ* of Ahiram is a local graphic peculiarity, which did not last long, since it is absent from both earlier and later inscriptions. The backward thrust of the bottom horizontal stroke of *beth* in Yeḥimilk and 'Abda' is also an ephemeral graphic fad, somewhat analogous to the backward thrust often found at the bottom of a cursive Hebrew *beth* of today (originating in ligatures, as pointed out to me by Julian Obermann). It cannot be used as an argument for an early date for these inscriptions relative to the others of our group, especially since other letters, such as *mem*, are distinctly later in type.

[15] Maisler was led to lower his dates unduly for Shipiṭ-Ba'al and the latter's immediate precursors by his insistence on the identification of the latter with the *Si-pi-it-ti-bi-'i-il* who paid tribute to Tiglath-pileser III about 738 B. C. (or a little earlier). However, the name appears again in the fifth century as that of a king of Byblus, while it also appears among Canaanite names in the Amarna Letters from the early fourteenth century B. C. as *Sipṭi-Ba'al*. Moreover, the script of the late ninth and eighth century in Phoenicia and Syria is very well known, and no such archaisms appear in any Northwest-Semitic inscriptions belonging to the middle of the ninth century and later. There is, accordingly, no reason whatever to give up the secure synchronisms with early Bubastite Egypt.

than there is today. All scholars made numerous mistakes. No new material approaches in significance the mass of tablets from Ugarit (Ras esh-Shamrah), which will immensely facilitate all future investigation in the field of the early Northwest-Semitic dialects.[16] It is true that some scholars, including particularly Albrecht Goetze, have insisted on the autonomy of Ugaritic and have been inclined to minimize the relationship existing between Ugaritic and South Canaanite.[17] Our observations on the inscriptions will bring out our own position.

I. INSCRIPTION ON THE SARCOPHAGUS OF AHIRAM (EARLY TENTH CENTURY B. C.).[18]

ארן · זפעל · [א]תבעל · בן אחרם · מלך גבל · לאחרם ·
אבה · כש<ב>תוהז[19] · בעלם · ואל · מלך · במלכם ·
וסכן ·

בס<כ>נם · וחמא · מחנת · עלי<על>[20] · גבל ·
ויגל · ארן ·
זן · תחתסף · חמר · משפטה · תהתפך · כסא · מלכה ·
ונחת · תברח · על · גבל · והא · ימח ספרה · לפף ·
שבל[21]

The coffin which[22] [It]tobaal, son of Ahiram, king of Byblus, made for his father as his ab(o)de in eternity. And if[23] any king or any governor

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[16] The number of verbal coincidences between Ugaritic and Byblian texts is very remarkable, as we shall see below. Of course, in judging them we must remember that the Ugaritic epics of Baal, Keret and Aqhat (Dan'el) were composed farther south, in Phoenicia proper, as is clear from the place-names and the divine names which they contain. I do not deny that their language has been modified somewhat by the Ugaritic dialect in which they are written, but this can hardly have been true of their vocabulary as a whole.

[17] See especially Goetze, *Language*, 17 (1941), 127 ff. I have been intending for several years to reply in detail; for the present cf. my discussion of method in *Cath. Bib. Quar.*, 1945, 14-18.

[18] In dealing with these inscriptions I shall not attempt to repeat what my precursors have established, except where it has been disregarded more recently; for details students must examine the literature on each inscription.

[19] Hitherto the reading of the text, בשתה, has been accepted by all except Ronzevalle, who queried the final *he* and proposed בשתר (*op. cit.*, pp. 21 f.), which he equated with Heb. סתר, "place of concealment, shelter." However, if all remaining cases of *he* and *resh* are compared, it becomes clear that it is just as easy to complete the character above by filling out two horizontal strokes as it is to fill out the head of the *resh*, which would occupy the same position with reference to the surviving traces. Moreover, Ronzevalle's etymology is impossible phonetically. It is true that the verb *šīt* has now been well established for older Canaanite from Ugaritic, and that Friedrich's proposal to vocalize *kī šāta-hu ba-'ōlam(i)* (*Mélanges Dussaud*, I, 43, below), i. e., "when he put him in eternity (= the tomb)," remains possible. However, in view of the certain omission of a letter in the sixth word following ours and the highly probable omission of a whole word immediately afterwards, my proposed reading *ka-š⟨ib⟩tih(u)* = Heb. *kə-šibtô*, "as his dwelling," becomes almost certain, in my judgment. Both Hebrew and Ugaritic provide close analogies to this expression. Note especially the phrase in the prayer of Solomon, מכון לשבתך עולמים, "a dais for thy dwelling (= enthronement) for ever" (I Kings 8: 13 and II Chron. 6: 2) and the phrase *ks'u ṯbty(h)*, "throne of my abode (sitting)," etc., in Ugaritic literature (e. g., V AB, F: 15). Note Accad. *šubtu*, "dwelling," for **šibtu*.

[20] Since 1926 I have felt that the haplographical ellipsis of *'l* must be assumed here, but it is the preposition, not the verb, which was accidentally omitted by the stonecutter. There can no longer be any doubt that the spelling עלי is correct for the perfect of a verb *tertiae yodh* but false for the preposition. Final *yodh* appears in these tenth-century documents in the perfects בני (twice) and חוי, "he restored" (*pi'el*); cf. Harris, *A Grammar of the Phoenician Language*, 1936 (cited hereafter as *Grammar*), p. 45. Pronounce *banaṣ*, etc.; the diphthong remained uncontracted because during the Late Bronze Age, when diphthongs were being contracted in Canaanite (Ugaritic, Amarna, most Egyptian transcriptions of the New Empire), the final short vowel had not yet been elided and the form was pronounced *bánaya*. The preposition appears as על in Ahiram, Yeḥimilk, Abibaal and Shipiṭ-Ba'al; see n. 43 for its occurrence in Elibaal. Whether it was pronounced *'al* or *'alê* or both, as in Hebrew, we cannot say; in any case the original diphthong had been contracted in Late Bronze. "To go up against" is the normal Hebrew expression for "attack"; for the use of a perfect in the protasis and imperfects in the apodosis see Gesenius-Kautzsch, *Hebräische Grammatik*[26], p. 519, § 159 n.

[21] This word has been a stumbling block, but the reading *resh* for the second letter may be considered as almost certainly wrong after my discussion, JPOS VII, 122 ff., and especially after Ronzevalle's treatment (*loc. cit.*). On the other hand, my attempt to read מתבל must be ruled out completely, not only because of the epigraphic traces, but also because the evidence against the use of a preposition *min*, "from," in Canaanite increases constantly (see n. 42).

[22] The relative translation is now certainly to be preferred to the demonstrative; see especially Friedrich, *Mélanges Dussaud*, I (1939), 39-47.

[23] The rendering "if" is now accepted by nearly all translators; the alternative identification with the Hebrew preposition *'el*, "to," encounters very serious difficulties, since the preposition in question has not been found hitherto in either Ugaritic or Phoenician, and since the resulting syntax is very awkward. The explanation of the form is still uncertain; I adhere to my

or any army commander attacks Byblus and exposes [24] this coffin, let his judicial scepter be broken, let his royal throne be overthrown,[25] and let peace [26] flee from Byblus; and as for him, let a vagabond(?) [27] efface [28] his inscription(s)!

Ia. GRAFFITO ON THE SIDE OF THE TOMB SHAFT OF AHIRAM.

(1) לדעת · (2) הן יפד ש לך · (3) תחת זן

(1) Attention! (2) Behold, thou shalt come to grief [30] (3) below here!

II. INSCRIPTION OF YEHIMILK (MIDDLE OF TENTH CENTURY B. C.).

1. בת · זבני · יחמלך · מלך גבל
2. ירח׳את [31] · חוי · כל · מפלת · הבתם
3. אל · יארך · בעלשמם · ובעל<ת> [32]
4. גבל · ומפחרת · אל גבל
5. קדשם · ימת · יחמלך · ושנתו
6. על גבל · כמלך · צדק · ומלך
7. ישר · לפן · אל גבל · ק׳דשם׳

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tentative identification of the word with Heb. *'ûlâi*, "perhaps, if," which suits the context here exactly as it does in such passages as Gen. 18: 24.

[24] The form is a characteristic imperfect with *waw* consecutive, agreeing in tense with the previous perfect עלי; here, then, we have the form which Harris missed (*Grammar*, p. 39). In this connection it may be added that the Nora text from the ninth century adds to the examples of the Phoenician perfect with *waw* consecutive cited by Harris (*BASOR*, No. 83, 19).

[25] As has already been pointed out by several scholars (cf. Ginsberg, *Orientalia*, 1936, 179) this passage agrees almost word for word with I AB, vi: 28 f., which forms the final cola of the tricolon *l-ys' 'alt ṯbtk l-yhpk ks'a mlkk lyṯbr ḥṯ mṯpṯk* = "May he (El) tear out the pillars (?) of thy dwelling, || may he overturn the throne of thy kingship, || may he break thy judicial staff!" As Torrey and Ginsberg have pointed out, the Phoenician gender of the nouns is feminine, agreeing with Ugaritic and Accadian against Hebrew.

[26] The meaning of the passage has been clear since 1927; cf. *JPOS* VII, 125. The word *nḥt* (i.e., *nôḥât* < **naubatu*), "peace," corresponds to Heb. *mənûḥâh*, which has just this meaning, e.g., I Kings 8: 56); it occurs as *nḫt* also in Ugaritic (V AB, D: 47 = Keret II, vi: 24), where *l-nḫt l-kḫṯ drkt* should be rendered "on the peaceful throne of authority," resolving the characteristic hendiadys.

[27] The last two words have been an enigma ever since the publication of the text. The latest suggestion is Gaster's (*Iraq*, VI, 140, n. 222), reading לפת תבל, "from the face of the earth," or the like. If it were not for the fact that my reading must be given up, as shown effectively by Ronzevalle (*loc. cit.*), I should be strongly inclined to accept it. לפת I derive directly from *lpp* (Aram., Accad., Arab. *lpp*, "to wind") in the sense of "wend (one's way)"; cf. also Arab. *liff, lafîf*, "crowd, mob." The word *šbl* can scarcely be separated from Heb. *šəbîl*, Aram. *šəḇîlâ*, Arab. *sabîl*, "road, way," so *lópep šabîl*, or the like, would mean "wayfarer, vagabond." It may be added that the word *šbl*, "road," is found in two Edomite (so!; cf. the independent conclusions of Noth, *Zeits. Deutsch. Pal.-Ver.*, 1938, 277-304, and myself, *Archiv f. Orientf.*, XII, 385 f.) names of places in the Shishak List, lines 73-76; one is *Šblt Ngby(t)* (so!; the inserted *r* is vertical dittography), "Southern Roads," the other is *Šblt Wrkyt*, "Northern Roads" (cf. Heb. ירכתי צפון, "extreme north").

[28] *Ymḥ* can only be jussive singular, *qal* or *nif'al*; indicative sing. and jussive plural would be written *ymḥy*.

[29] This reading was established by Vincent, *Rev. Bib.*, 1925, Pl. VIII, and p. 189, n. 1.

[30] Vincent's interpretation, *hsypd lk* from a supposed verb *ypd*, "look out for thyself!", is unparalleled linguistically. He should have adhered to his own combination with Heb. *pîd*, "misfortune, ruin," which offers admirable sense; cf. the Arabic *fâda, yafûdu, faud*, "to die, dying, death," and *fâ'ida*, "to suffer a stroke" (from *fu'âd* = Ugar. *p'ed*, "heart, etc."), *fâ'id*, "dying." Note also the possible Aramaic cognates *pwd* (*pauḍâ*, "error") and *pdd*, "to err, wander." The syntax is easy; note the frequent use of the impersonal construction with the masc. sing. passive in Arabic (Wright-de Goeje, II, pp. 268 f., § 133) and with the intransitive or passive followed by ל in Hebrew (König, *Lehrgebäude*, III, pp. 351 ff., §§ 323 f.), indicating a possible vocalization *yupâd*.

[31] I have no hesitation whatever in reading *he* instead of *zayin* here—the photographic reproduction in *Fouilles de Byblos*, I, Pl. XXXI, 2 seems particularly clear, since the lower end of the vertical shaft of *he* appears on it distinctly. Since the זאת hitherto read here by all scholars is grammatically unintelligible, it must in any case be discarded. In favor of reading the emphatic demonstrative and personal pronoun ending in *t* here is the fact that the plural *hmt*, "they, them, those," Ugar. *hmt*, is common in Phoenician, and that the forms *hwt*, "he, him," and *hyt*, "she, her," have now been proved to exist in Ugaritic (Gordon, *Ugaritic Grammar*, § 5. 10 f., p. 23). Previously these forms were known to exist in South Arabic (Sabaean *hwt* and *hyt*, whence Ethiopic *we'ětû* and *ye'ětî*, from **hûwatu* and **hiyati*, respectively), while parallel forms were known from Egyptian (*ẖwt* and **ẖyt* beside *ẖw* and *ẖy*) and Accadian (*šu'atu* and *ši'ati* beside Old Accad. *su'a* and *si'a*). The alternation of *waw* and *aleph* in these forms is, of course, well illustrated by Arabic *huwa* and *hiya* beside Heb.-Phoen. *hû'* and *hî'*. Phoenician thus possessed the following personal pronouns of the third person singular: masc. *hû'*, fem. *hî'*, emphatic masc. *hû'at*, fem. **hî'at*.

[32] Since no such deity as "Baal-Gebal" is otherwise

(1) The temple which Yeḥimilk, king of Byblus, built—(2) it was he who restored [33] the ruins of these temples. (3) May Baal-shamem [34] and Baal-(ath)-Gebal (4) and the assembly [35] of the holy gods of Byblus (5) prolong the days of Yeḥimilk and his years (6) over Byblus as a rightful king and a true (7) king [36] before the h[oly] gods of Byblus! [37]

III. INSCRIPTION OF ABIBAAL ON STATUE OF SHISHAK (CIR. 925 B. C.).

1. [מש (?) זן] ובזא [38] . אבבעל . מלך [גבל .
ביחמלך (?) [39] .
2. מלך.] גבל . במוצרם [40] . לבעל[ת . גבל .
אדתו [41] . תארך .
בעלת . גבל . ימת . אבבעל . ושנתו.] על גבל

(1) [The statue (?) which] Abibaal, king of [Byblus, son of Yeḥimilk (?), (2) king] of

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known, while Baalath-Gebal appears repeatedly in these texts, we may rest assured that the omission of the final *tau* was a slip of the stone-cutter, presumably due to the fact that he had just carved the name *Ba'al-shamêm*.

[33] On the meaning and etymology of this word (vocalized approximately *ḥawwáy* for old **ḥáwwaya*) see *JBL*, 1944, 223, n. 108.

[34] *Ba'al-shamêm*, "Lord of Heaven," is evidently the name of Baalath-Gebal's consort. The problem of this deity is far from settled; cf. most recently Eissfeldt, *Zeits. Alttest. Wiss.*, 1939, 1-30, and Levi della Vida, *JBL*, 1944, 1-9 (where on p. 6, n. 24, my remarks in *Archaeology and the Religion of Israel*, p. 72, have been misinterpreted).

[35] It has been pointed out by several scholars (cf. Ginsberg, *Orientalia*, 1936, 179) that *mpḫrt 'l Gbl* is paralleled closely by Ugaritic *mpḫrt bn 'Il*.

[36] It has not yet been observed that there is a striking verbal parallel between this passage, in which the abstract nouns *ṣdq* and *yšr* are successively combined with *mlk*, "king," and Keret I, i: 12 f.: *'aṯt ṣdqh l-ypq mtrḫt yšrh* = "Let him find his rightful wife, || his true spouse."

[37] Note the length of the sentence in lines 3-7. There can be no doubt that Phoenician prose could be quite involved, though probably less so than the rhetorical prose of the Canaanites of the Bronze Age, whose case-endings and modal endings served to avert confusion. Note the extraordinary length of some sentences in the Prologue of the Code of Hammurabi. However, it is also to be observed that subordinate clauses, so familiar in Hebrew prose of the Deuteronomic type (seventh-sixth centuries B. C. [cf. *BASOR*, No. 73, 21]), appear to be missing.

[38] The character before *aleph* is lost, except for the slanting stroke on its upper left, which Clermont-Ganneau's photograph shows to be drawn a little too far down in the line by Montet (*Rev. Bib.*, 1926, Pl. VI; *Byblos et l'Égypte*, p. 53). There is only one letter which this stroke will fit, a *beth*. I do not, therefore, hesitate to restore *z-yb'* instead of the *yṯn'* or *nš'* which have hitherto been popular (though neither *nun* nor *shin* fits the traces). The form *yb'* is causative perfect in Phoenician; the fact that this form has not yet been found in such an early text is doubtless accidental. The meaning of Heb. *hēbî'*, "he caused to come, he brought," fits perfectly into the text; see n. 40.

[39] Abibaal may have been an older brother of Elibaal, or possibly a younger brother of Yeḥimilk; other possibilities are naturally not excluded. For the correct explanation of the previously enigmatical [ ]ביח in the Elibaal inscription as "son of Yeḥimilk" see the discussion by Maisler, *op. cit.*, pp. 172 f. The phonetic reduction of *bn*, "son," to *b* occurs also in the cuneiform alphabetic inscription from about the thirteenth century B. C. published by Yeivin (cf. *BASOR*, No. 99, p. 21) and in the two recently published Byblian inscriptions (our Nos. V and Va); Maisler has gone too far in the enthusiasm of discovery in making further identifications, but I am very much impressed with his Phoenician(!) source for the name *Bidqar* in *Bindikiri*, which occurs with other *Phoenician* names in an Assyrian tablet of the seventh century. As H. L. Ginsberg has correctly pointed out, *n* remains unassimilated before laryngeals.

[40] This reading seems certain to me, despite Maisler's objections (*loc. cit.*). The traces in Clermont-Ganneau's photograph of the squeeze are indeed rather wide for a narrow character, but they are much too narrow for two characters; *ṣade* is in fact the widest archaic Phoenician character, as a rule. Moreover, the horizontal traces at the right agree only with the horizontal strokes in the tenth-century *ṣade*, for which cf. the arrow-head of Ruweiseh (Guigues and Ronzevalle, *Mélanges de l'Université St.-Joseph*, 1926, 325-358) and the Gezer Calendar (cf. *BASOR*, No. 92, 20). In fact, all that is missing is the short vertical stroke at the left, which may be present, but is not clear in the photograph. Clermont-Ganneau, Lidzbarski, Montet and their successors are thus correct in reading *Miṣrêm*. The only personal name containing the consonants *M..rm* known to me is *Milkirâm* (*Malkîrâm*), but the traces preclude any such reading.

[41] This word is not to be read *'adotto* (or the like), as supposed hitherto, but unquestionably *'adottew* (or the like), corresponding to the Hebrew masculine *'adōnâw* (older *'adōnēw* [*BASOR*, No. 92, 22, n. 27]), "his lord." In Hebrew the masculine appears (except in the case of *'adōni*, "my lord," which was evidently used in addressing men in order to obviate any confusion with *'adōnai* as an appellation of Yahweh) nearly always (virtually always with pronominal suffixes) as an honorific plural (*pluralis majestatis*) applied both to God and to men. We should, therefore, expect the same usage where the feminine form of the word is applied to a goddess. However, just as in Hebrew we find the masculine singular used with the first person in Punic *donni*, "my lord" (Plautus); the derived Greek Ἄδωνις is phonetically somewhat obscure. The suffixed *waw* is used exactly as in שנתו, "his years."

Byblus, [br]ought from[42] Egypt for Baal[ath-Gebal, his lady. May Baalath-Gebal prolong the days of Abibaal and his years] over Byblus!

IV. INSCRIPTION OF ELIBAAL ON STATUE OF OSORKON I (CIR. 915 B.C.).

1. ומשׂ . זפעל . אלבעל . מלך . גבל . ביח[מלך . מלך . גבל]
2. [לב]עלת . גבל . אדתו . תארך . בעלת[גבל]
3. [יתת . א]לבעל . ושנתו . על[43] [גבל]

(1) [The statue] which Elibaal, king of Byblus, son of Yeḥi[milk, king of Byblus,] made (2) [for Ba]alath-Gebal, his lady. May Baalath[-Gebal] prolong (3) [the days of E]libaal and his years over [Byblus]!

V. INSCRIPTION OF SHIPIṬ-BA'AL (END OF TENTH CENTURY B.C.).

1. קר . זבני . שפמבעל . מלך
2. גבל . בן אלבעל . מלך . גבל
3. ביחמלך . מלך . גבל . לבעלת
4. גבל . אדתו . תארך . בעלת גבל
5. ימת . שפמבעל . ושנתו . על . גבל

(1) The wall which Shipiṭ-Ba'al, king of (2) Byblus, son of Elibaal, king of Byblus, (3) son of Yeḥimilk, king of Byblus, built for Baalath-(4) Gebal, his lady. May Baalath-Gebal prolong (5) the days of Shipiṭ-Ba'al and his years over Byblus!

[42] The preposition *b* was regularly used in the sense of "from" both in Ugaritic and in Phoenician. For Phoenician examples cf., e.g., ואל יעמסן במשכב ז עלת משכב שני, "and let it not be removed from this resting-place to another resting-place" (Eshmun'azar, lines 5 f., 7 f.) and בשרדן נגרש האדם הא . . . [(ninth-century inscription from Nora in Sardinia; *BASOR*, No. 83, 19), "[That ma]n (shall) be banished [ ] from Sardinia." Heb. *min*, "from," is not attested in Phoenician any more than in Ugaritic; the parade example, מנם, in the Carthage Tariff (CIS 167:6) is unquestionably the indefinite pronoun *mnm*, common in both Ugaritic and Phoenician; cf. H. L. Ginsberg, *Jour. Pal. Or. Soc.*, 1934, 250, 1935, 182. The few remaining instances (cf. Harris, *Grammar*, p. 120) may easily reflect assimilated or dissimilated forms of *b*, since they always precede two nasals and a labial. They may also reflect dialectal usage.

[43] Some scholars have read [גבל] עלי, but the sloping stroke after *lamedh* can only belong to *gimel*—certainly not to *yodh*; see Mor.tet's photograph and drawing.

Va. INSCRIPTION OF 'ABDA' (ABOUT 900 B.C.).[44]

[ל] ועז בדא . בכלבי . תויז [צר]

[Belonging to] 'Abda', son of Kalbai,[45] the p[otter.]

VI. INSCRIPTION ON A BRONZE SPATULA (ABOUT 1000 B.C.).[46]

[- -]י . לעזירז בעל
תשעם . שלם [.] סנ[47]
נשבת . אם נחל
תנחל . מנשתך[48]
עלך . ומנשת
לי[49]

(1) [ ]ay (says) to 'Izrî-Ba'al, (2) Dost thou seek (?) reconciliation? Withdraw, (3) let us

[44] See Dunand, *Byblia Grammata*, pp. 152 ff. My reading agrees with his, except that I prefix a *lamedh*, in accordance with the analogy of similar Palestinian vase inscriptions; cf. Maisler, *op. cit.*, p. 168.

[45] For ב instead of the full בן, "son of," cf. n. 39. The name *Klby* appears also in the Ugaritic texts; on it cf. *BASOR*, No. 82, 47, n. 26.

[46] For the literature on this spatula see my latest study, *BASOR*, No. 90, 35-37, Dunand, *Byblia Grammata*, pp. 155-7, and Torczyner, *Leshonenu*, XIV (1946), 158-165. My reading remains unchanged since my last previous study of the text in 1943 except for giving up the *n* in line 2, in accordance with the traces; there is insufficient space for any letter, so we must probably restore a stroke of separation.

[47] This cannot be *pe* with Dunand and Torczyner, since all hitherto published examples of tenth-century *pe* are rounded, most of them achieving an arc or even a semicircular appearance. The angular *pe* noted by Dunand in his table (*Byblia Grammata*, p. 160) is actually rounded; cf. his photograph, *ibid.*, Plate XVIa, line 1, and note that its stance is as different as possible from that of the supposed *pe* on the spatula. The supposed *pe* of the spatula is identical in form with the two examples of *gimel* in lines 4 and 5; moreover, this same form of *gimel* appears also in Ahiram (several times, a little more vertical in stance) and elsewhere (e.g., Shipiṭ-Ba'al, line 4, end (sloping slightly to the left instead of the right).

[48] Almost certainly plural; cf. my remarks, *ibid.*, p. 36, n. 9.

[49] Dunand supplies initial *'ayin*, reading לי[ע], but there is no trace of it either in his photograph or his hand-copy; moreover, the *lamedh* is so far (relatively speaking) to the right of the *lamedh* in the previous line as to leave no room for it. Torczyner reads it as though there were no question about the reading. In my opinion the context virtually excludes it.

settle (our case)![50] If thou dost truly (4) inherit (the property), thy offering(s) (5) shall be incumbent upon thee, but my offering(s) (6) shall belong to me.

The latest translation, following previous ones by Dunand, Obermann and the writer, has been proposed by Torczyner, who interprets the text as follows:[51]

(1) What is incumbent upon me (to pay) 'Azar-Ba'al (2) is 90 (shekels)—pay the silver[52] (3) (which) thou hast taken[53] (borrowed)! If thou dost truly (4) inherit (my portion also of the inheritance), thou shalt pay (as compensation the money which thou hast taken) from[54] (the yield up to the present of) thy flax (fields which thou hast inherited); (5) and from (the yield of the) flax (fields which belong to my inheritance, which thou dost acquire from me now, shalt thou pay the debt) (6) which is incumbent upon me![55] (In other words, this shall be the price of the inheritance which thou art now acquiring from me.)

These tenth-century inscriptions have two particularly significant aspects: (1) the light which they shed on the question of Ugaritic-Phoenician relationship; (2) the light which falls from them on the orthography of Hebrew.

As pointed out repeatedly above, there are many similarities in language between Phoenician and Ugaritic. Since Byblus is only about 100 miles in a straight line from Ugarit, and since only some four centuries separate the two groups of inscriptions, this is to be expected. If the opinion of most Ugaritic scholars, that Ugaritic is a Canaanite dialect, is right, the close relationship between them would be inevitable. Illustrations of verbal similarity have been given above, in notes 19, 25, 26, 31, 35, 36, 48, etc. Among the most striking parallels of grammatical significance are the new pronoun *h't* = Ugar. *hwt*, the use of *b* instead of *min* in the sense of "from" in both, the use of the *ifte'al* in both, the feminine gender of the words for "scepter" and "throne," against Hebrew usage; Ginsberg, Gordon and others have pointed out other parallels from later Phoenician inscriptions.[56] One which should be emphasized strongly is the fact that both employ *bl*, not *l'*, for negation.[57] Of course, the Canaanite of the Amarna letters from Byblus resembles Ugaritic still more closely, since they are contemporary instead of being separated by four centuries or more.

The spelling of the tenth-century inscriptions from Byblus is just as phonetic as that of the Canaanite stele of Kilamuwa, from the latter part of the ninth century, but it is more archaic in several respects. E.g., the final *yodh* of perfect *tertiae-infirmae* verbs is no longer written in the third person masculine singular in Kilamuwa, while the pronominal suffix of the third person singular is no longer *he*, but *yodh*, in Kilamuwa, just as in all later Phoenician inscriptions except apparently the royal Byblian inscriptions from the sixth-fourth century (Shipiṭ-Ba'al III,[58] Yeḥawmilk,[59] Bat-no'am[60]). It is not yet certain whether Harris is right in assuming a special Byblian dialect (*Grammar*, p. 51) or whether I was correct in treating these forms as archaizing (*JAOS* 60, 420). In any case some revision of current assumptions with regard to the suffixed pronominal elements is necessary. It is a mistake to suppose that the late Byblian inscriptions indicate the suffix of the first person singular by *yodh*, except in the case of לי and לפי, both of which can be explained as historical survivals from the beginning of the first millennium, in which the *yodh* still appears with

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[50] This passage vividly illustrates Prov. 18:18; the word *yaḥbiṭ* is used there in exactly the same sense as I attribute to it here: "(the lot) settles (legal strife)." For details see my discussion, *loc. cit.*

[51] The following English paraphrase is based on Torczyner's vocalized text and Hebrew paraphrase on p. 165 of his paper. I know of no ancient legal documents where so much must be read into the text to make sense.

[52] Reading כ[סף]; see n. 47.

[53] Deriving *nšbt* from the verb which appears as *nsb*, "take, bring," in Aramaic. This is possible only if the Syriac verb corresponds to a Hebrew **nšb* and to Arab. *nšb* (with Brockelmann, *Lex. Syr.*[2], *ad voc.*), but not if the Aramaic corresponds to Arab. *nsb*, as I strongly suspect.

[54] The improbability of treating the initial *mem* as the preposition will be seen from n. 42.

[55] Reading לי[ע]; cf. n. 49.

[56] Cf. Ginsberg, *Orientalia*, 1936, 179 f.; Gordon, *Ugaritic Grammar*, pp. 88 ff.

[57] Against Obermann, *JBL*, 1946, 233-248.

[58] See Dunand, *Fouilles de Byblos*, I, 31 f.

[59] See most recently, utilizing an additional fragment from Byblus, Dunand, *Bulletin du Musée de Beyrouth*, V, 57-85.

[60] See most recently Dunand, *Fouilles de Byblos*, I, 31.

genitives, just as in Ugaritic. Yeḥaw-milk twice employs קל for *qôlî*, "my voice," where the noun is unquestionably singular. On the other hand, we find in these texts עצמי, "my bones," and רבתי, i. e., *rabbôtai*, or the like, not *rabbatî*, "my lady"; for the *pluralis majestatis* see above, n. 41. Moreover, לפני and עלי both follow the plural pattern, just as in Hebrew. All cases of affixed *waw* in the late Byblian inscriptions represent the third-person masculine singular suffix with plural nouns (e. g., שנתו, ימו) except possibly in the case of זרעו, where Hebrew parallels suggest a singular noun; however, this is not certain, and in view of cases like קל and עין for *'ênê*, I find it hard to believe that the form was pronounced *zar'ô*, as in Hebrew. In all these instances, except possibly the last, we have full agreement with Byblian practice in the tenth century, a fact which appears to weight the balance in favor of my theory of archaistic spelling.

In view of the completely phonetic consonantism of the tenth-century Byblian inscriptions, which was doubtless shared by the orthography of the Phoenicians in general at that time, it seems to me highly improbable that the Israelite orthography of the tenth century was any less phonetic, since the cultural influence of Phoenicia on Israel was demonstrably at its height then. In fact my studies of the Gezer Calendar [61] and the Oracles of Balaam,[62] confirmed subsequently by examination of such tenth-century texts as II Sam. 22 = Psalm 18, have fully confirmed this expectation. Naturally, owing to minor differences between Phoenician and Hebrew morphology, one cannot expect entire agreement in the consonantal spelling.

In conclusion I append a summary list of the kings of Byblus known to have flourished after cir. 1100 B. C.

Zakar-Ba'al	c. 1075 B. C.
. . . . .	
Ahiram	c. 1000
Ittobaal (son of Ahiram)	c. 975
Yeḥimilk	c. 950
Abibaal (son of Yeḥimilk?)	c. 930
Elibaal (son of Yeḥimilk)	c. 920
Shipiṭ-Ba'al I (son of Elibaal)	c. 900
. . . . .	
. . . . .	
Shipiṭ-Ba'al II (Sipitti-bi'il)	c. 740
Ormilk I (Uru-milki)	701
Milk-asap (Milki-ašapa)	c. 670
. . . . .	
. . . . .	
Shipiṭ-Ba'al III [63]	c. 500 ?
. . . . .	
Ormilk II	
Yiḥar-Ba'al (son of Ormilk II) [64]	
Yeḥaw-milk (son of Yiḥar-Ba'al)	c. 450 ?
. . . . .	
El-pa'al [65]	c. 360
'Ozi-Ba'al	348
Addir-milk	c. 340
'Ayyin-El	333

[61] *BASOR*, No. 92, 16-26.
[62] *JBL*, 1944, 207-233.

[63] Cf. n. 58.
[64] See Dunand, *Bulletin du Musée de Beyrouth*, V, 75.
[65] For these kings see most recently Dunand, *Fouilles de Byblos*, I, 407 ff.

See the online article entitled "Ramses II Re-Dated by Biblite Evidence" , by Damien F. Mackey

Paper 4 Phoenician inscriptions on tiles from the Tell el-Yahudiyeh temple of Ramses III confirm our 757-726 BC dates for that pivotal 20th dynasty king.

Immanuel Velikovsky, in attempting to justify his 4th century dating of Ramses III (in his 1977 volume "*Peoples of the Sea"*), argued the case that tiles from Ramses' Tell el Yehudiyeh temple are inscribed with Greek letters from the Ptolemaic period. We argue instead 1) that the letters are Phoenician, not Greek, and 2) that they date from the middle of the 8th century.

On page 6 of his book *Peoples of the Sea*, in a section entitled "*Greek Letters on Tiles of Ramses III*" (pp. 6-12), Velikovsky describes a collection of fresco tiles which, according to him, validate his early 4th century dating of Ramses III. The reader might do well to read the half dozen pages in this section, though online access is limited to a later edition of this book, where pages 6-12 of the original volume now appear verbatim as pages 22-29 of the later edition, the extra page resulting from the inclusion of plates inserted from elsewhere in the original. We quote here the introductory paragraphs from this section, with Nile Delta map added by this author:

Nile Delta

Tell el-Yahudiya, or "The Mound of the Jew," is an Arab village east of the Delta, twenty miles northeast of Cairo on the road to Ismailia. Over ninety years ago the Swiss Egyptologist Edouard Naville excavated there the ruins of a palace of Ramses III. Tiles, colored and glazed, once adorned its walls. They were found in great numbers on the site by traveling scholars and also by Emil Brugsch in the service of the Egyptian Department of Antiquities, before Naville, assisted by F.L. Griffith, came to dig there. The tiles have rich designs, mostly of flowers, and some bear the hieroglyphic name of Ramses III. On the reverse side of these tiles are found incised signs: these are apparently the initials of the craftsmen who produced them, inscribed before the tiles were fired.

> There was no doubt that the signs on many tiles in the palace of Ramses III at Tell el-Yahudiya were Greek letters. "The most noticeable feature is that several of the rosettes have Greek letters at the back, evidently stamped on during the process of making," wrote T.H. Lewis, orientalist and art expert, to whose judgment the tiles were submitted.
>
> But how could Greek letters have been used in the days of Ramses III, early in the twelfth century before this era? The Greek alphabet was derived from the Phoenician or Hebrew much later; no traces of it have been found in Greece, on the islands, or in Asia Minor before -750. (Peoples of the Sea 6-7)

So far, so good. Velikovsky has just argued our case. The dates of Ramses' temple tiles, and thus the dates of Ramses III himself, must be reduced from early in the 12th century to the approximate year 750 BC. Unfortunately Velikovsky did not know to stop at this point. His objective was to date Ramses III to the 4th century BC, not the middle of the 8th and certainly not the 12th. So he proceeded for the next five pages to document the conflicting opinions of scholars on the evidence from the tiles. All were seemingly convinced, apparently influenced by the opinion of a single "art expert", that the letters inscribed on the back of the Ramses' temple tiles were Greek and possibly as late as the Ptolemaic age, while the presence of Ramses' name on some tiles, and the clear association of all tiles with the Ramses III temple, clearly date the tiles to the first quarter of the 12th century BC, this assuming the accuracy of the traditional Egyptian chronology. The impasse among early scholars was left unresolved and remained so till the time Velikovsky wrote his book, with the famed revisionist supporting the "expert" opinion regarding the 4th century dating of the tiles. To his credit Velikovsky wisely ends the discussion by quoting the Egyptologist Naville:

> "Light will be thrown on the question someday."

We agree, and a source of light has now arrived. Already we have established that Ramses governed Egypt in the years 757-726 BC, thus reducing the scope of the problem in question. All we need is some confirmation that the letters on the back of the temple tiles date from the approximate year 750 BC, and we can rest our case. And that confirmation is ready at hand.

We begin by emphasizing the fact that the scholars quoted by Velikovsky all lived in the early decades of the 19th century, when the science of the development of the Phoenician, Hebrew, and Greek scripts was in its infancy. No scholar today

would venture those same opinions with such conviction. And today the question would not be centered exclusively on the evolution of the Greek alphabet. It would also focus on the ethnicity and linguistic training of the artisans who manufactured the tiles, and the purpose for which the letters were inscribed on the back of the tiles. Let me explain.

Velikovsky was of the opinion that the craftsmen who fabricated the tiles were merely inscribing their initials, thus letters with which they had intimate familiarity. But it is incomprehensible why they would do that, knowing that the backs of the tiles would soon be affixed to the walls of the temple, permanently out of sight. We think instead they were using the alphabet to number the tiles, or perhaps to designate the intended position of a given tile in the fresco for which it was created, thus a means by which those tiles might be removed from the kiln and placed in a predetermined position. And we think the workmen were likely linguistically untrained, in possession of a rudimentary knowledge of the alphabet employed, and not the least bit concerned whether anyone other than themselves could identify the letters. And finally, surprisingly, we do not think the letters were actually from the Greek alphabet. We think they are Phoenician, and if Greek we insist that they originate from a very early stage in the development of the Greek script, around the year 750 BC as Velikovsky stated.

And why are we of this opinion? Because in the edition of the book to which we have provided the earlier link, Velikovsky provides nine line drawings of a sampling of the tile letters, and photographs of front and back of five of the tiles.

And we immediately recognized the Phoenician equivalent of four of the photographs and could identify the probable Phoenician equivalent of all but two of the line drawings, all based on research done in the course of writing our previous paper, where we examined a multitude of Phoenician inscriptions dated in the approximate time frame 820-680 BC. From the photographs at least, we determined conclusively that four of the letters on the Ramses' tiles were all

Phoenician. They were extremely precisely inscribed, with due allowance made for the fact that the tile fabricators were not professional scribes. Any residual doubt that the tile letters were Phoenician stems from the fact that we are here exposed to only nine line drawing of the letters, five with accompanying photographs, and four others for which no photograph is provided. And as we will soon see, the accuracy of the line drawings is highly suspect. At a later date we will perhaps return to the subject and obtain photographs of more of the actual tiles to confirm our analysis. In the meantime we must make do with the meager evidence provided. We do want to at least look at the photographs, and make a few comments regarding how accurately the line drawings reflect, or rather do not reflect, what the photographs reveal.

Velikovsky provides front and back views of five tiles. We have to assume that he has chosen samples that best support his argument, i.e. those inscribed with letters most clearly representative of Greek capital (uncial) letters of late provenance. Thus we are presented samples supposedly depicting the Greek letters alpha, epsilon, iota, lambda, and chi. We want to look at them all. We begin with the alpha.

In the lengthy dialogue which fills much of the seven pages of text, Velikovsky several times mentions the fact that the presence of the Greek letter alpha, in its existing form on a least one tile, constitutes a strong argument for dating all of the Greek letters to the 4th century. A case in point is the quote attributed to the respected Egyptologist Emil Brugsch

> "The Greek letters, and especially alpha, found on the fragments and discs leave no room for doubt [ne laissent aucun doute] that the work was executed during the last centuries of the Egyptian Empire and probably in the time of the Ptolemies, but the matter becomes more difficult if we ask who the author of this work was [an allusion to Ramses III]." So wrote Emil Brugsch. (PS 8) [bracketed explanation added]

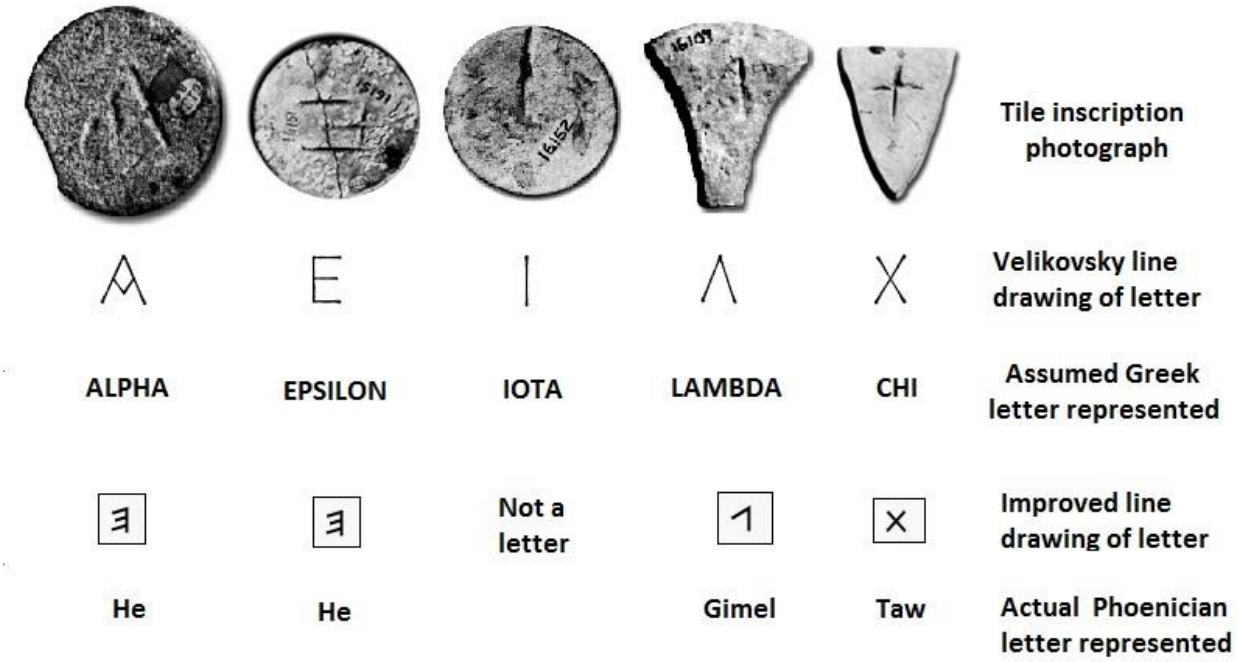
In view of the comment by Brugsch we take a closer look at the "aleph" in question. Thankfully, Velikovsky provides a large fairly high resolution photograph. We assume this is the only tile backed with an aleph. [In the instance where two tiles are backed by a "lambda", Velikosky provides photographs of both.] We reproduce below a close-up of the back of the "alpha" tile.

The first thing the reader should note is that there exists, on the back of this tile, four line impressions that form a backward facing capital E, rotated slightly counter-clockwise. Notice that the vertical shaft or stem of the E and the three arms do not meet at right angles. Also notice that the stem extends slightly beyond the beginning of the lowest arm, forming what we might call a "tail". All three arms appear to be of the same length, or approximately so, but in this example at least, each of the arms is noticeably shorter than stem, even without including the tail. The reader should block out of consideration the vertical mark that appears to extend from near the end of the middle arm to the central part of the upper arm. That is merely a flaw in the back surface of the tile. Remember that this tile was once somehow cemented in place to form a wall or floor fresco. All of the five temple tiles shown by Velikovsky exhibit some damage.

When you have followed these simple instructions you will be looking at the actual inscribed letter. Properly oriented it looks like this - . The "alpha" tile letter is actually an absolutely perfect representation of a Phoenician "he" from the approximate year 750 BC, a sign consistently represented in transcriptions as an "h".

Now let's look at the line drawing made of this same tile impression as it appears on the extreme left of Velikovsky's list of nine "Greek letters" - . Notice that the angle between the vertical shaft and the upper and middle arms of the backward E has been reduced marginally to give the "A" a more convincing shape; the flaw on the back surface has been incorporated into the sign; and the lower arm of the E, which was admittedly faint in the photograph, has been deleted entirely. Equally disturbing is the fact that the length of the upper arm of the backward E has been lengthened slightly to correspond to the length of the stem, thus creating the two supporting arms of the A, and the combined length of the two parts of the "broken" cross hatch of the A, which on the tile is at least equal to the length of the stem, is now only half that length. And in the process a perfectly formed Phoenician "He" has been deliberately transformed into a Greek "Aleph" with a damaged cross-hatch. And this is what convinced Brugsch that all of the lettered tiles from the temple of Ramses III were of 4th century BC date?

And what can we say about the remaining four photographed tiles, those represented in positions #2-4 and #9 of Velikovsky's sequence of 9 line drawings. For convenience we have summarized our opinion in a single diagram, shown below.



Corrected interpretation of the five inscribed tiles whose photographs were provided in Velikovsky's book "Peoples of the Sea"

Velikovsky's Greek epsilon is actually another Phoenician "he", not so well inscribed as the "alpha" but clearly recognizable. Of course we have had to rotate the tile 180 degrees for proper viewing, and we still need to rotate it counter-clockwise a few degrees to view it with the stem oriented vertically, as it would appear in Phoenician inscriptions. But once we do we see that the three arms meet the shaft at less than 90 degrees, and the tail, missing from the photo, can be explained by the chipped surface of the tile precisely where the tail of the "he" is expected. In this instance the upper and lower arms are longer than the stem, somewhat atypical, but as we will see momentarily, arm length is not a critical aspect of the letter.

The other three tiles are not at all problematic. Velikovsky's "iota" is nothing but a crack in the tile extending from the center to the outer rim. His "lambda" and "chi" are perfect examples of Phoenician "gimel" and "taw", as we will see in a

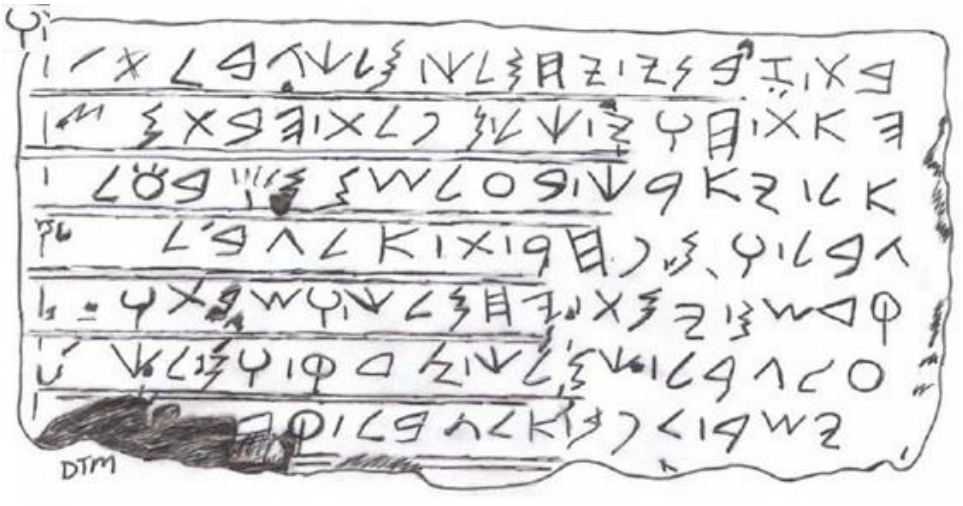
moment. The above diagram demonstrates conclusively that items #1, #3-4 and #9 on Velikovsky's list of supposed Greek letters are essentially identical with Phoenician script from the late 9th through early 7th centuries. Which leads us to ask how we might identify the temple tile letters for which Velikovsky has provided line drawings only, those in positions #5-8 on his list. May we suggest, initially, that the tiles with the "M" and the "C" should be viewed after an appropriate rotation, in which case letters #5-8 in the line drawing list should be emended from 1) to 2) or even to 3) . May we suggest further, based on example 3), that the first three letters be identified as Phoenician **shin** , **ayin** , and **waw** . With the "waw" we are assuming that the stem was either not visible due to damage, or faint, and thus ignored, as was the case with the "alpha" examined earlier. Without the photographs we cannot be overly dogmatic, but our suggestions are perfectly reasonable, particularly when we make allowances for the bias of those who constructed the line drawings, who seem to have had a tendency to modify the visual appearance of the tile letters to better represent them as Greek uncials.

And in case the critics accuse us of bias in our representations of the Phoenician letters, we might add that our depictions of the Phoenician letters, with one exception, have been copied verbatim from the internet Wikipedia site entitled The Paleo-Hebrew Alphabet. The exception is the waw, which here has an unusual appearance. We have copied it from an actual Phoenician inscription, one belonging to the Phoenician king of Byblos named Yehimelek, and dated in our previous paper to the approximate year 760 BC, only three years before the beginning of the reign of Ramses III. We have copied the inscription from the Biblia-Hebraica Blog.

The Phoenician inscription of Yehimelek will serve a dual purpose. On the one hand it will assist in confirming our representations of the Phoenician letters on the tiles already discussed. And it will serve additionally to assist us in identifying the one letter in Velikosky's list of nine that we have yet to discuss - letter #8, which in his line drawing represents the Greek letter "tau" (T). A glance the the Yehimelek inscription may provide an answer. Both the Phoenician letters Gimel and Tau have the requisite lines intersecting at right angles or close to right angles. Either could be mistaken for a T given some damage to the tile surface

that either adds to or detracts from the normal appearance of the sign. That is particularly true of the Phoenician “taw”. Should a chipped surface remove any one of the four “spokes”, you have a T. And it is particularly appropriate, if true, that the Phoenician “taw” should be mistaken for a Greek “tau”, since historically they both had the “t” sound and the one might well have evolved into the other.

The inscription of Yehimelek, king of Gebel (later Byblos)



We leave it to the reader to find the two "he"
the four "shin"
the three "ayin", twice the second letter in name of Ba'al
the five "gimel" alway occurring as the first letter in the name of Gebal, ancient Byblos
the four "waw"
and eight "taw"

We leave the matter there. As always, let the reader decide if we are right.

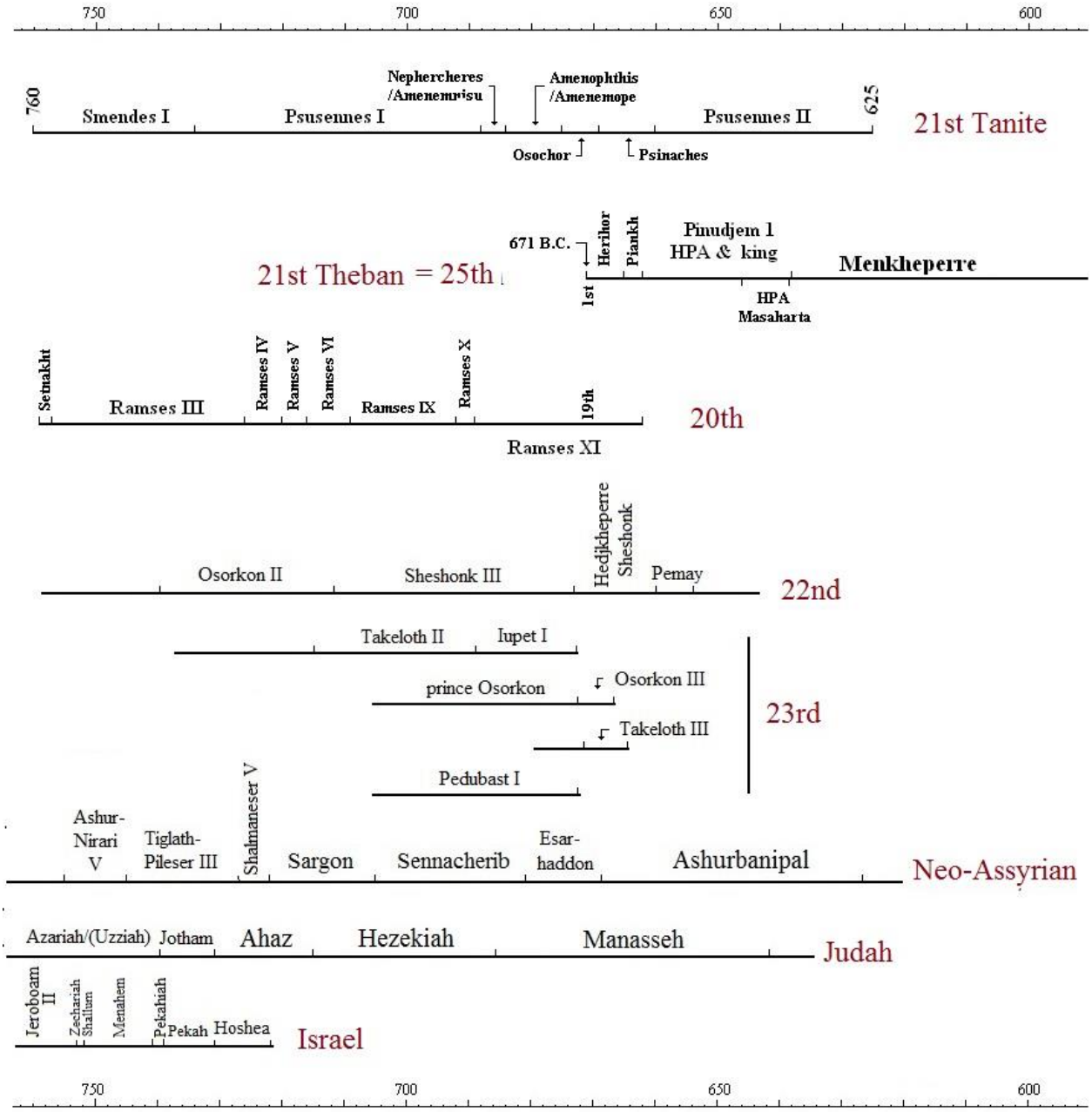
Paper #5 The fact that Egypt was governed by five overlapping dynasties in the century following the Santorini mega-explosion (ca 760-650 BC) is confirmed by the Harris papyrus, the neo-Assyrian annals of Tiglath-Pileser III, Sennacherib & Ashurbanipal, and by the papyrus documenting the travels of Wenamun.

In our last paper we outlined the revised history of Egypt during the late 9th through early 7th centuries BC, providing timelines for the 20th, 21st Tanite, 21st Theban, 22nd, and 23rd dynasty kings in two separate charts. Here we combine those charts into one, and add timelines for the neo-Assyrian kings and the rulers of Israel and Judah, all in the approximate time frame 760-650 BC. The resulting Figure 1 fills the whole of the following page. We suggest that the reader download and print that page to assist in following the content of this paper.

Our aim in this paper is to validate the absolute dates assigned to those dynasties. There are six topics discussed, numbered consecutively. All six rely heavily on points we have already made in our Egyptian books one and two, this for the sake of readers unfamiliar with the content of those lengthy volumes. All six introduce new material relevant to the discussion. In all cases the Figure 1 timelines are fixed in place by linking them with well dated Phoenician, neo-Assyrian and biblical events. The topics also function to provide links between the timelines themselves. The next paper has the identical purpose and structure.

We emphasize at the outset the fact that all four Egyptian timelines stand or fall together. They were not determined by independent processes, but by a procedure unique to this *Displaced Dynasties* series. Unlike all other revisionist efforts familiar to this author, we have chosen to begin the re-writing of history at a secure historical juncture, the year 404 BC, the concluding year of the 27th (Persian) dynasty. From there we carefully moved back in time, one dynasty at a time, first moving the 26th dynasty forward 121 years to overlap the 27th, then moving earlier dynasties forward, one at a time, to fill the vacated temporal space, a domino effect. Only after our Figure 1 timelines were firmly in place, near the end of our second book, did we discover the Louvre and Berlin stele which confirmed their accuracy. Thus, before we add one further detail to this paper, the dates of the four named dynasties are already firmly “written in stone”. We have not the slightest doubt concerning their accuracy.

Figure 1: Revised history timelines of the kings of Israel, Judah, Assyria, and Egypt in the approximate time frame 750-650 BC.



In this paper we have selected for discussion six subjects that are particularly well suited to the purpose at hand. We itemize them in point form.

1) The fact of a major catastrophe in the Mediterranean world between the 19th and 20th Egyptian dynasties has long been known, based in part on a brief historical entry affixed to the famous papyrus Harris (which documents the appearance of Setnakht, founder of the 20th dynasty), and based additionally on the multiple invasions of Egypt by Sea Peoples and assorted tribes of Libyans that took place in the early years of Ramses III (documented on the walls of Ramses' Medinet Habu temple).

On pages 113-117 of chapter five in our book 3 (*The Genealogy of Ashakhet Part 1: From Amarna to Troy*) we identified the likely cause of the catastrophe, the cataclysmic explosion of the mega-volcanic island of Santorini, north of Crete, near the entrance to the Aegean Sea. We dated the event to the year 765 BC. In that same book 3 the whole of chapter six (pages 118-140) was devoted to outlining the consequences of that volcanic eruption for nations bordering the Mediterranean, including Egypt. It was truly a holocaust. Ninety percent of the population of the Levant died, the result of gigantic tidal waves and torrents of molten ash. The civilization of the Empire Hittites ended, as Anatolia was overrun by warlike migrants. Egypt survived, blessed by the Nile, but the 19th dynasty descendants of Ramses II were devastated by wave upon wave of immigrants. Syrian survivors, and multiple tribes of Libyans seeking sustenance, were enticed to invade Egypt, lured by the richness of the Nile valley, its flood plains constantly replenished by the resurgent Nile. Sea Peoples, identified by names such as Denyen, Ekwesh, Lukka, Peleset, Shekelesh, Sherden, Teresh, Tjeker, and Weshesh invaded Egypt, but they were quickly repulsed via naval battles and onland skirmishes. Most were shipped back to the coast of the Levant, where over time they repopulated that coastline, if not the Syrian hinterland. In the latter half of this paper, and in the next, we will have cause to visit several Tjeker communities, and at least one Peleset village, both recent arrivals on the post-765 BC Levantine coast.

In the last paragraph we mentioned Syrian survivors attacking Egypt in the immediate aftermath of the Santorini explosion. That information is sourced in the initial lines of the historical section of the papyrus Harris. We quote below the relevant lines as translated in 1906 by James Henry Breasted on pages 198-9 in volume IV of his classic *Ancient Records of Egypt* (BAR):

> "The land of Egypt was overthrown from without, and every man was thrown out of his right; they had no "chief mouth" for many years formerly until other times. The land of Egypt was in the hands of chiefs and of rulers of towns; one slew his neighbour, great and small. Other times having come after it, with empty years, Irsu ('a self-made man'), a certain Syrian (Kharu) was with them as chief (wr). He set plundering their (i.e., the people's) possessions. They made gods like men, and no offerings were presented in the temples.
>
> "But when the gods inclined themselves to peace, to set the land in its rights according to its accustomed manner, they established their son, who came forth from their limbs, to be ruler, LPH, of every land, upon their great throne, Userkhaure-setepenre-meryamun, LPH, the son of Re, Setnakht-merire-meryamun, LPH. He was Khepri-Set, when he is enraged; he set in order the entire land which had been rebellious; he slew the rebels who were in the land of Egypt; he cleansed the great throne of Egypt; he was ruler of the Two Lands, on the throne of Atum. He gave ready faces to those who had been turned away. Every man knew his brother who had been walled in. He established the temples in possession of divine offerings, to offer to the gods according to their customary stipulations." (Breasted, BAR IV sect 398-9 pp 198-9)

The reader should not be dissuaded from accepting our identification of Santorini as the cause of the dispersion of the Sea-Peoples, or our dating the event in the approximate year 765 BC., by multiple claims to the contrary proposed by historians and scientists alike, who date the most intensive of the sequence of eruptions of this volcano either in the year range 1630-1600 BC (based on *calibrated* radiocarbon results) or a century later, to the Late Minoan IA period, ca. 1500 BC, based on archaeological excavations, several of which register the presence of 18th dynasty artifacts on the rim of the caldera.

The scientific estimate can be discounted entirely. How can you radiocarbon-test organic material remains when there are no material remains to test? The entire island was literally vaporized. It is now estimated that upwards of 60 km^3 (cubic kilometers) of the island dissipated in the span of moments, and almost certainly even the surface layers at the rim of the caldera were blown away. If any organic

matter was unearthed by the explosion, it could conceivably originate from occupations that preceded the mega-eruption by decades, even centuries.

This absence of organic remains explains why radiocarbon tests are conducted tens or even hundreds of miles away, in the debris layers left by the tsunami waves generated by the Santorini explosion. One such test, conducted by Bruins, van der Plicht, and MacGillivray, and published in 2009 in the journal *Radiocarbon* (vol 51, pp 397-411) is illustrative and representative. The title tells the tale.

> THE MINOAN SANTORINI ERUPTION AND TSUNAMI DEPOSITS IN PALAIKASTRO (CRETE): DATING BY GEOLOGY, ARCHAEOLOGY, 14C, AND EGYPTIAN CHRONOLOGY

We recommend the reading of this article, if only because it is typical of recent research on the dating of the Santorini event. The three authors here did their primary research in the ash laden debris on Palaikastro in Crete, almost 100 miles south east of Santorini. Their tests were conducted on animal remains found in the debris. They were surprised at the consistency of the radiocarbon dates determined here and on the surface of the Santorini site itself, all in the range 3350 ± 10 BP, suggesting that the animals died ca 1300 BC. They up that figure by about 300 years by calibrating the results, a self-authenticating procedure applied universally to radiocarbon results by scientists, a process discussed and deprecated elsewhere by this author (see page 12 in paper #4 on our inerrantbible.com website).

Before we comment further on the radiocarbon results we note that the Palaikastro authors also incorporated in their analysis some discussion of the archaeological results, summarized in a table we have reproduced on the following page.

Table 1 Simplified conventional scheme of archaeological material-cultural relationships between Thera, Crete and Egypt in the 2nd millennium BCE, forming the basis of archaeo-historical dating linked to the Egyptian historical chronology.[a]

Thera (Santorini)	Crete	Egypt (Dynasties)	Years BCE (Egyptian chronology)
Late Cycladic IIIB	Late Minoan III	XIX	~1295–1186
Gap	Late Minoan II	XVIII	~1550/1539–1295
	Late Minoan IB		
Eruption	Late Minoan IA		
Late Cycladic IA			
Middle Cycladic	Middle Minoan III	Hyksos XV/ XIII–XIV	~1801/1759–1550/1539
	Middle Minoan II	XII	~1979/1937–1801/1759

These results are quite typical. Archaeologists have determined, correctly we believe, that the Santorini event must postdate the early decades of the 18th dynasty, this because early 18th dynasty remains were unearthed by the Santorini eruption, as found at Akrotiri and elsewhere on the rim of the volcano. But the existence of those remains does not prove that the mega eruption took place during the 18th dynasty. Far from it. It proves only that the explosion postdates the construction of these 18th dynasty remains. Even today archaeologists unearth buildings and artifacts dated thousands of years in the past. Tourists visiting these excavation sites are not so foolish as to conclude that the events which unearthed those remains (the spades of the archaeologists) must be assigned the same date as the remains themselves. The reasoning is flawed. Almost certainly multiple occupation levels were torn up and vaporized by the force of the Santorini explosion, exposing some of the 18th dynasty remains. If the explosion happened hundreds of years later than the 18th dynasty, there is no doubt that some 18th dynasty buildings still existed on the site, and survived the explosion. And we are positive that the Santorini explosion postdates the early 18th dynasty by over two centuries.

A glance at the chart above reveals the fact that the Palaikastro archaeologists believe otherwise. Their chart shows that they date the Santorini eruption in the middle of the 18th dynasty, during the Late Minoan IA period, and assume that the resulting caldera remained unoccupied for several hundred years, until re-occupied near the end of the Late Minoan III era, the time of Ramses III. There is

absolutely no evidence to substantiate that opinion. And that theory flies in the face of the balance of archaeological evidence, which suggests that the occupants of Santorini knew in advance that the volcano was about to explode, and vacated the island. While many of these evacuees may have died regardless, many others would be anxious to return the moment the conditions normalized. The caldera of Santorini remains today an island paradise. Even if the core of the island was blown away, the remaindered would beckon its former residents to return. The suggestion that the island remained uninhabited for several hundred years is not just unreasonable, it is absurd.

Our revised history believes instead that the explosion took place at the end, not the beginning of the two hundred year gap in the Palaikastro chart. During the two hundred year gap between Minoan IA and Minoan III the 18th and 19th dynasties lived a normal existence on the island. But when Santorini exploded in the year 765 BC all evidence of their presence was obliterated by the force of the volcanic eruption. The assumed two hundred years of unoccupied residence in a caldera is a complete fiction. And the former residents, who had fled the island prior to the eruption, probably did attempt to return. But if so they were quickly overrun by Phoenicians, Sea Peoples looking for a home, many of whom were transported out of Egypt by Ramses III in his 5th year. The fact that Phoenicians were the dominant group in the repopulation of the caldera is not contested by scholars. The evidence clearly favors our revised history. But archaeologists have turned a blind eye to it.

Let us be clear. If we accept the archaeological evidence at face value, apply some common sense, and adopt the chronology employed by the traditional history, we should date the eruption of the Santorini volcano at the end of the 19th dynasty, not the middle of the 18th. And since the 19th dynasty is dated in the time frame 1308-1194 BC in that schema, the eruption should be dated 1194 BC, or thereabouts and the existing 18th dynasty remains about 250 years earlier, slightly over mid-way through the 18th dynasty.

Our revised history has not altered the relative dating of these events significantly. We have simply lowered all of the dates in question by approximately 430 years, arriving at the date 765 BC for the explosion and the

approximate year 1000 BC for the existing 18th dynasty remains. [Note that our revised history dates the 18th dynasty in the time frame ca 1050-870 BC.] And most of that 430 year number is explained by our Figure 1, where we have moved the 21st Tanite and 22nd dynasties forward to join the 20th, eliminating approximately 300 years from the Egyptian timetable. Another 121 years was eliminated when we began our revision, moving the 26th dynasty forward to overlap the 27th.

Having discounted (or rather, reinterpreted) the archaeological evidence which dates the Santorini event to the approximate year 1500 BC, we return to our discussion of the radiocarbon determination of the event, an uncalibrated dating to the vicinity of the year 1300 BC to which was applied a questionable calibration, resulting in dating the explosion to the approximate year 1600 BC. How do we explain these results? Again we refer the reader to our most recent discussion of radiocarbon dating at our inerrantbible.com website, where we remark on the fact that atmospheric conditions are a critical determining factor in the interpretation of radiocarbon results, often attributing dates which are thousands of years too old. In that paper #4 we were discussing the presence of water vapor in the atmosphere. Here we are concerned with volcanic ash and other pollutants associated with volcanos. Who knows what effect an ash-polluted atmosphere, which began long before the mega-eruption and likely endured for decades afterward, had on the radiocarbon levels ingested by plants and animal at the time. Here we quote a single source, the online web-info radiocarbon site which emphasizes the fact that even today, the still active Santonini volcano influences radiocarbon determinations, even in spite of atmospheric conditions which have long since returned to normal. We leave the interested reader to investigate the matter further.

> Radiocarbon samples which obtain their carbon from a different source (or reservoir) than atmospheric carbon may yield what is termed **apparent ages**. A shellfish alive today in a lake within a limestone catchment, for instance, will yield a radiocarbon date which is excessively old. The reason for this anomaly is that the limestone, which is weathered and dissolved into bicarbonate, has no radioactive carbon. Thus, it dilutes the activity of the lake meaning that the radioactivity is depleted in comparison to 14C activity elsewhere. The lake, in this case, has a different **radiocarbon reservoir** than that

of the majority of the radiocarbon in the biosphere and therefore an accurate radiocarbon age requires that a correction be made to account for it.

One of the most commonly referenced reservoir effects concerns the ocean. The average difference between a radiocarbon date of a terrestrial sample such as a tree, and a shell from the marine environment is about 400 radiocarbon years (see Stuiver and Braziunas, 1993). This apparent age of oceanic water is caused both by the delay in exchange rates between atmospheric CO2 and ocean bicarbonate, and the dilution effect caused by the mixing of surface waters with upwelled deep waters which are very old (Mangerud 1972). A reservoir correction must therefore be made to any conventional shell dates to account for this difference. Reservoir corrections for the world oceans can be found at the Marine Reservoir Correction Database, a searchable database online at Queen's University, Belfast and the University of Washington. Human bone may be a problematic medium for dating in some instances due to human consumption of fish, whose C14 label will reflect the ocean reservoir. In such a case, it is very difficult to ascertain the precise reservoir difference and hence apply a correction to the measured radiocarbon age.

Spurious radiocarbon dates caused by volcanic emanations of radiocarbon-depleted CO2 probably also come under the category of reservoir corrections. Plants which grow in the vicinity of active volcanic fumaroles will yield a radiocarbon age which is too old. Bruns *et al.* (1980) measured the radioactivity of modern plants growing near hot springs heated by volcanic rocks in western Germany and demonstrated a **deficiency in radiocarbon of up to 1500 years** through comparison with modern atmospheric radiocarbon levels. Similarly, this effect has been noted for plants in the bay of Palaea Kameni **near the prehistoric site of Akrotiri, which was buried by the eruption of the Thera volcano over 3500 years ago** (see Weninger, 1989). The effect has been suggested as providing dates in error for the eruption of Thera which has been linked to the demise of the Minoan civilization in the Aegean. One **modern plant** growing near the emanations **had an apparent age of 1390 yr**. The volcanic effect has a limited distance however. Bruns *et al.* (1980) found that at 200 m away from the source, plants yielded an age in agreement with that expected. They suggested that the influence of depleted CO2 declined rapidly with increasing distance from the source. Radiocarbon discrepancies due to volcanic CO2 emissions are a popular source of ammunition for fundamentalist viewpoints keen to present evidence to show that the radiocarbon method is somehow fundamentally flawed. (emphasis added)

We encourage the interested reader to search for and follow the references mentioned. We are not surprised by the fact that over a thousand years can be added to radiocarbon results by changes in atmospheric conditions, via a phenomenon known as “radiocarbon depletion”, whereby plants and animals are breathing in CO2 that has not been overly exposed to the cosmic radiation which

creates the radioactive C^{14}. We are only surprised that radiocarbon testing at Santorini and its environs has only inflated the age of the explosion by upwards of 500 years.

2) The emergence, around the year 760 BC, of four Egyptian dynasties rather than one, is more than adequately explained by the circumstances which prevailed in the approximate year 765 BC.

The 20th dynasty king Setnakht, and his son the future Ramses III, already had a foothold in the south of Egypt when the mega-volcanic island of Santorini exploded in ca 765 BC, effectively ending the tenure of the 19th dynasty king Merenptah and his sons, though they did struggle for a time to regain their legacy. The Mediterranean world had for many years prior been suffering the effects of repeated less drastic eruptions of the Thera volcano, foretastes of the coming cataclysm. The kings of Libya are known to have invaded Egypt already in the fifth year of Merenptah, five years prior to the final upheaval, without doubt motivated by the destructive effects of multiple foreshocks, and lured by the prospects of the secure fresh waters of the Nile. The intrusion of Libyan emigrants almost certainly intensified after the 765 BC cataclysmic eruption, though the only documented instances are dated to the 5th and 11th years of Ramses III, more than a decade after the eruption. The claim is made in the Harris papyrus that Ramses repulsed those Libyan invaders, ruthlessly slaughtering many, making heaps of their remains. But it is admitted in that same papyrus that tens of thousands were spared. Not only were Libyan immigrants drafted into the Egyptian army, but others, including select tribal leaders and their followers, were provided sanctuary and sustenance. The following excerpt from the Harris papyrus is a case in point. We are here reading Breasted's translation in BAR IV sect. 405 on pages 201-02.

> Behold, I will inform you of other things, done in Egypt since my reign. The Libyans and the Meshwesh were dwelling in Egypt, having plundered the cities of the western shore, from Memphis to Kerben. They had reached the great river on both its banks. They it was who plundered the cities of Egwowe during very many years, while they were in Egypt. Behold, I destroyed them, slain at one time. I laid low the Meshwesh, the Libyans, the Esbet, the Keykesh, the Shai, the Hes and the Beken; they were overthrown in their blood and made heaps. I turned them back from trampling the border of Egypt. **I carried away those whom my sword spared, as numerous captives, pinioned like**

> **birds before my horses, their wives and their children by the ten-thousand, their cattle in number like hundred-thousands. I settled their leaders in strongholds in my name. I gave to them captains of archers, and chief men of the tribes, branded and made into slaves, impressed with my name; their wives and their children were made likewise**. I led their cattle into the house of Amon; they were made for him into herds forever. (BAR IV sect 405 pp 201-2) (emphasis added)

Besides this admission that Libyan immigrants had gained a foothold in the country with the tacit approval of the 20th dynasty kings, we can only guess at how many Libyans accessed Egypt via the western deserts, encountering no resistance whatever. It is beyond the ken of this author why Egyptologists insist that the Libyan invasions that gave rise to the 22nd dynasty only took place in 945 BC, and not in the early years of the reign of Ramses III (1182-1151). How can they deny the obvious, that the arrival of the 22nd dynasty Libyans can be securely dated to the first decade of Ramses' reign. That situation does not change when we adjust the dates for Ramses III, and insist his reign began around the year 757 BC, not 1182 BC. And it follows that we can securely date the arrival of the 23rd dynasty Libyans to that same mid-8th century era.

The invading Libyans were no doubt led by royalty whose kingships began in Libya, among whom we can name at least three, Sheshonk, Osorkon, and Takeloth, the assumed progenitors of the emerging 22nd dynasty. The predecessors of the 23rd dynasty, which may have included the aforementioned Takeloth, were also ethnic Libyans. They almost certainly accompanied their 22nd dynasty counterparts, though their presence in Egypt was first noted with the emergence of Takeloth II. The 22nd dynasty arrivals appear to have domiciled in the north, primarily in the western Delta. The 23rd dynasty immigrants likely settled in the central Nile, and later, following the death of Ramses III in 726 BC, in the Theban area, and even further south, ***where they appear in the biblical record, and in Assyrian annals, as kings of Melukkha***.

So much for the emergence of the 22nd and 23rd dynasties. There is no mystery associated with their arrival in Egypt. But what about the 21st Tanite dynasty kings? How and when did they arrive in the environs of Tanis? From whence did they come? Again there is no mystery. These Tanites from the outset bore Egyptian names, and were devotees of the Theban god Amun. They functioned

from the beginning as nothing more than a branch of the Theban religious cult of Amun, using the chaotic conditions that prevailed in Egypt post 765 BC to expand their cult and acquire the political stature they had always coveted. Historians rightly call these 21st dynasty rulers "priest/kings" because in truth they functioned as both, perhaps even more as priests than kings. In our next paper we will visit with one of the earliest of these Tanite priest/kings, whose titles agree precisely with our theory regarding their origins.

3) Assuming that four dynasties of nomarchs, rather than one, emerged from the ashes of the Santorini cataclysm (pardon the pun), it should follow that these dynasties would cooperate in some undefined ways in the governance of the country. In particular we should expect intermarriage to have taken place to some significant extent.

It is well known that several 21st dynasty kings and dignitaries, and not a few members also of the 22nd dynasty, are associated in the monuments in some undefined way with the 20th dynasty. Psusennes I and his son Ankhefenmut at times identified themselves with the Ramesside kings by adopting the hyphenated names Ramses-Psusennes and Ramses-Ankhefenmut. Various attempts have been made to explain this intrusion of the name Ramses into a 21st dynasty context, omitting that which is most reasonable, namely, that the Tanite priest/kings ruled in close association with the Ramesside kings, often entering into marital alliances. The noted Egyptologist Kenneth Kitchen referenced this close alliance between the 21st and 20th dynasty kings in his commentary on these names, but could only leave the matter unresolved, still captivated by the mistaken conviction that the 21st dynasty followed on the heels of the 20th. His comments are still worth reproducing:

> It has not escaped attention that Psusennes I appears to claim some connection with the preceding, Ramesside, dynasty. Thus, one fragmentary block from Tanis bears the cartouche 'Ramesses-Psusennes, Beloved of [Amun]', which recurs on a ring-bezel from the burial of Wen-djeba-en-Djed in the tomb of Psusennes I. Furthermore, in room 3 of his Tanite tomb, Psusennes I made provision for the burial of a prince who was doubtless a son that predeceased him, given the probable long duration of Psusennes' reign. In brief form, this man was called simply 'the King's Son, Ankhefenmut'; but on the end of his sarcophagus were set out his full name and titles, calling him (inter alia): 'Bodily King's Son whom he loves, Ramesses-Ankhefenmut.' These compound names, Ramessses-Psusennes and Ramesses-Ankhefenmut, are wholly in the style of those of

> the sons and successors of Ramesses III in the 20th Dynasty, and suggest that Psusennes I and his son claimed a link with the Ramessides, a claim which was not taken up by their successors. TIP 41

Before we proceed we should perhaps interject here that in the preceding discussion relating to the 21st dynasty king Psusennes I, and also in subsequent discussion where Psusennes I and II are mentioned, we continue to use the number designation followed by the scholarly community, even though, as argued later in this paper, we believe that the traditional history has reversed the true order of these kings. The king named Psusennes I, with son named Ankhefenmut, was actually the second of the namesake kings, and technically should be named Psusennes II. We will discuss that situation in greater depth in our next paper.

We continue now with discussion regarding the use of compound names involving Ramses, this time by the 22nd dynasty kings, a fact even more perplexing to Egyptologists. Upwards of a dozen dignitaries mentioned in 22nd dynasty inscriptions bore the title "King's Son of Ramses", for which there is only one reasonable explanation - the bearer of the name was a son, a grandson, or at minimum a near descendant of a king named Ramses, even if only on the maternal side of the family. And once again Ramses III would be the most likely 20th dynasty relative. But now the connections, at least for advocates of the traditional history, are even less comprehensible than the use of Rameside names by the 21st dynasty Tanites. Moreso since some of these references are from late in the 22nd dynasty. At least one, that of a "King's Son of Ramses Pashedbast", was found among the relics in the burial chamber of Harnakht, son of Osorkon II (see Kitchen, TIP 93 (p. 118). Two others are even later. We quote Kitchen on the details. But we should read critically. He is discussing the sons of Shoshenk III, three of whom are certain, the fourth and fifth less so. It is only because their names appear on monuments contemporary with Sheshonk III that they are included in Kitchen's list:

> A fourth son was a "King's Son of Ramses", Commander of All Troops, Great Chief of [—], Takeloth (C), son of the Lord of the Two Lands, his mother being Djed-Bast-es-ankh', whose activity at Busiris is denoted by a donation-stela of Year 18, most likely of Shoshenq III. A fifth possible prince was the 'High Priest of Amenresonter, King's Son of Ramesses, Mek-prince of Pawer ..., Army-leader, Padebehenbast', on a donation-stela of Year 28 of Shoshenq III, probably from Kom el Hisn. TIP 305.

Takeloth and Padebehenbast are not sons of Sheshonk as Kitchen thinks. They are probably, as their title clearly suggests, sons of a 20th dynasty king Ramses. [It is possible, of course, that Sheshonk III married a daughter of a Ramesside king, and that these children were born to him by that wife.] There are yet other dignitaries who bear the identical title, but these examples are sufficient to draw attention to the problem. What else should we conclude from these references than the fact that the 22nd dynasty pharaoh Sheshonk III, whose reign in the traditional history is dated 837-798 BC, named his sons after a 20th dynasty Ramesside king, a dynasty that is dated 1184-1087 BC. in that same (grossly errant) chronological schema. How likely is that? It totally defies reason why any monarch would identify his sons with any king of a *foreign dynasty* that died out 250 years prior. Yet apparently the incongruity of the situation does not unduly concern the noted Egyptologist.

Disturbing though the dual names are for conventional historians, the situation is possible, even probable, once we accept the fact that Ramses III reigned in the years 757-726 BC and Sheshonk III in the years 715-674. They are even more comprehensible once we realize that the 20th dynasty king Ramses XI, almost as renowned as Ramses III, was an associate of Sheshonk III for almost half the reign of that 22nd dynasty pharaoh (see our Figure 1). Let the reader decide which chronology is correct.

4) At least once in the time frame under discussion an Assyrian king acknowledged the fact that Egypt, his adversary, was governed by multiple kings. One such reference is enough to prove our case.

According to the traditional history the 25th dynasty consisted of at minimum four Nubian kings, Piankhi, Shabaka, Shabataka, and Taharka, each governing in succession, in the time frame 750-671 BC, the whole of Melukkha and Egypt. If true, we need to delete the entire contents of our four Egyptian books, all ten papers in the Mesopotamian section, and the four papers in the Additional Argument section. Instead, we choose to question the reliability of the traditional history, beginning by quoting a single excerpt from the annals of the Assyrian king Sennacherib (704-681 BC). The entry belongs to the annals which document his 3rd campaign (701 B.C.) where, in the midst of a description of his assault on the cities of Judah, he notes that Hezekiah, the king of Jerusalem,

> had become afraid and had called (for help) upon the ***kings of Egypt*** (Mus(u)ri) (and) the bowmen, the chariot(-corps) and the cavalry of the ***king of Ethiopia*** *(Meluhha)*, an army beyond counting - and they (actually) had come to their assistance. In the plain of Eltekeh (Al-ta-qu-u), their battle lines were drawn up against me and they sharpened their weapons. Upon a trust (-inspiring) oracle (given) by Ashur, my lord, I fought with them and inflicted a defeat upon them. In the melee of the battle, I personally captured alive the Egyptian charioteers with **the(ir) princes** and (also) the charioteers of the king of Ethiopia. (ANET 287-88)

In Appendix A, on pp 387-388 of book 1, we briefly discussed this inscription, and pointed out the incontestable fact that

> the idea of multiple kings of Egypt existing alongside of an independent king of Melukhkha [in the last decades of the 8th century BC] is entirely foreign to any historical situation that can be hypothesized for the late 25th dynasty, when either Shabataka or Shabaka is considered to have ruled all of Egypt including the lands southward as far as the 4th cataract.

Clearly this single annals entry absolutely contradicts the traditional history in which a single Ethiopian king rules the whole of Egypt. The author of this paper is, among other things, a mathematician, well aware that a lengthy and detailed proof of the accuracy of a theorem can be invalidated by a single counterexample. As here.

But we can do better than merely argue against the traditional history. We know precisely, based on a parallel description of this same Eltekeh battle in the Hebrew Bible, and from multiple mentions of a namesake Melukkhan king in the annals of the Assyrian kings Esarhaddon and Ashurbanipal subsequently, the identity of the "king of Melukkha" who fought the Assyrian army of Sennacherib in 701 BC., alongside his 20th, 21st, and 22nd dynasty associates. The Hebrew Bible calls him Tirhakah (2 Kings 19:9; Isaiah 37:9) and the Assyrian annals of Esarhaddon and Ashurbanipal call his namesake son Tarqu. And in our Figure 1 Tirhakah and Tarqu can be firmly identified as kings Takeloth II and Takeloth III, Libyan kings of the 23rd dynasty, resident in the upper reaches of the Nile, almost certainly south of Thebes, as described earlier in our point 2 on page 11. The interested reader is invited to follow our discussion related to Takeloth I & II in chapter 3 of book 1, pages 63-67.

5. Setnakht, the father of Ramses III and founder of the 20th dynasty of Egypt, began his reign by expelling from the Egyptian delta a group of Syrian invaders, led by a mercenary named Arsu/Arzu. Equating this Arzu and a Syrian king named Rezin, assists in validating the accuracy of our 760-757 BC dates for Setnakht.

In our initial remarks in section 1 of this paper (pages 3 & 4) we reviewed some of the evidence attesting the cataclysmic explosion of the mega-volcanic island of Santorini in the approximate year 765 BC and its consequences for Egypt. We quoted a section of the Harris papyrus translated in 1906 by James Henry Breasted documenting the fact that Egypt was invaded soon after by opportunistic Syrian rebels led by a certain Irsu, who were eventually driven from the country by Setnakht, father of Ramses III and founder of the 20th dynasty. At the conclusion to our second book we identified this Irsu/Irzu with the Syrian king Rezin. At the time we were rushed to publish the concluding chapter of that book and omitted some of the detail verifying the accuracy of the identification. Here we correct that omission.

It is clear from the quoted material on page 3 that Setnakht "slew the rebels who were in the land", which technically could include the rebel leader, though most interpreters think otherwise. Consequently, since the days when these opening lines were read by Egyptologists, scholars have been scrambling to determine the identity of Irsu/Irzu/Arzu, the Syrian. They have been painfully unsuccessful for obvious reasons. They date the transition period between the 19th and 20th dynasties to the approximate years 1195-1185 BC. The events in question actually took place in the time frame 765-760 BC. Small wonder their search was unsuccessful. Had they looked 430 years into the future, they would have been overwhelmed by references to the rebel Arzu. Rezin, the mercenary warmongering ruler of Syria, dated 754-732 by scholars, is infamous for his forays into foreign lands, his multiple attempts to expand his sphere of influence. We supply here a few details about this man we failed to document fifteen years ago when we published our second book.

In the concluding pages of *Piankhi the Chameleon* we did explain the fact that the book of Amos in the Hebrew Bible, in its entirety, was written during the sole reign of king Uzziah (767-750 BC), probably at the very beginning of that reign. Amos' dates the writing of book two years prior to an event it describes in its opening line as a ***raash***, a term commentators have weakly translated

"earthquake", but which we have interpreted as violent tremors and resulting tsunami generated from the eruption of Santorini which, in combination with torrents of molten ash, killed 90% of the population of the Levant (see Amos 5:3), including the remnant of the ruling houses of Syria, those of Hazael (842-796) and a long line of kings named Ben-hadad.

> "So I will send fire upon the house of Hazael,
> And it will consume the citadels of Ben-Hadad." (Amos 1:4)

This was no mere earthquake. It was remembered in the days of the biblical prophet Zechariah, writing 120 years after the fact, while predicting the violent upheaval of the earth in the days preceding the arrival of Messiah.

> And in that day His feet will stand on the Mount of Olives, which is in front of Jerusalem on the east; and the Mount of Olives will be split in its middle from east to west by a very large valley, so that half of the mountain will move toward the north and the other half toward the south.
>
> And you will flee by the valley of My mountains, for the valley of the mountains will reach to Azel, yes, you will flee just as you fled before the earthquake (***raash***) in the days of Uzziah, king of Judah. Then the LORD, my God, will come and all the holy ones with Him! Zechariah 14: 4,5

While Rezin is not mentioned explicitly in the book of Amos, there is not a single expositor of the book who does not firmly believe that his rise to power in the aftermath of the *raash* is implied in verse 1:5, which follows immediately the mention of the demise of the Hazael and Ben-Hadad ruling families.

> "I will also break the gate bar of Damascus,
> And cut off the inhabitant from the valley of Aven
> And him who holds the scepter, from Beth-eden;
> So the people of Syria will go exiled to Kir."
> Says the LORD.

Verse 1:5 is not a reference to the rise of king Rezin, but to his eventual demise. And along with the commentators, we can identify the three parties mentioned in the verse, Rezin the ruler in Damascus, king Pekah of Israel whose death apparently took place in "the valley of Aven", and Shamshi-ilu, the Assyrian turtanu whose birth and lifelong home in Bit-Adini is proverbial. Shamshi-ilu commanded the armies of a succession of neo-Assyrian kings, including Ashur-

dan III (774-756), in whose tenth year (765 BC) the sun was obscured by the ashes from the Santorini explosion, Ashur-Nirari V (756-745), and part of the reign of Tiglath-Pileser III (745-727).

In the year 732 BC, according to the Hebrew Bible:

> Rezin king of Syria and Pekah son of Remaliah, king of Israel, came up to Jerusalem to wage war; and they besieged Ahaz, but could not overcome him.
> At that time Rezin king of Syria recovered Elath for Syria, and cleared the Judeans out of Elath entirely, and the Syrians came to Elath and have lived there to this day.
> So Ahaz sent messengers to Tiglath-pileser king of Assyria, saying "I am your servant and your son; come up and deliver me from the hand of the king of Syria, and from the hand of the king of Israel, who are rising up against me."
> And Ahaz took the silver and gold that was found in the house of the LORD and in the treasuries of the king's house, and sent a present to the king of Assyria.
> So the king of Assyria listened to him; and the king of Assyria went up against Damascus and captured it, and carried the people of it away into exile to Kir, and put Rezin to death. (2 Kings 16:5-9)

The date of this incident does not come from the Hebrew Bible, but from the annals of Tiglath-pileser. Rather than detail the Assyrian references, we choose rather to quote from the excellent Wikipedia article which discusses, not the

> In c. 732 BCE, Pekah allied with Rezin, king of Aram and threatened Jerusalem. The prime reason for such a league was probably to protect their respective countries from another incursion of Tiglath-pileser III., who had compelled Menahem, in 738 B.C., to pay a large tribute. The two kings united their armies and attempted to coerce Ahaz of Judah into joining them. Pekah raided Judah and carried to Samaria a number of captives; but, rebuked by the prophet Oded and by some of the prominent men, he released them and sent them back. The united forces of Israel and Syria appeared before the walls of Jerusalem to demand its surrender. At this juncture Isaiah the prophet came to the support of Judah and her king. The allies had proposed to set upon the throne of Judah a son of Tabeel, probably one favorable to the alliance. Ahaz, however, knowing that Tiglath-pileser was within call, appealed to him for help.[7] Ahaz's "dread" of Rezin and Pekah, "Son of Remaliah" is recorded in the Immanuel prophecy in Isaiah 7:14 where the birth of a son (possibly Hezekiah[8]) is a sign of the defeat of both kings by the King of Assyria before the child is old enough to eat curds and honey and distinguish right from wrong. After Ahaz paid tribute to Tiglath-Pileser, the Assyrians sacked Damascus and annexed Aram.[9] According to 2 Kings 16:9, the population of Aram was deported and Rezin executed. According to 2 Kings 15:29, Tiglath-Pileser also attacked Israel and "took Ijon, Abel Beth Maacah, Janoah, Kedesh and Hazor. He

> took Gilead and Galilee, including all the land of Naphtali, and deported the people to Assyria." Tiglath-Pileser also records this act in one of his inscriptions.[10]
> Soon after this Pekah was assassinated by Hoshea, the son of Elah, who then took the throne. Tiglath-Pileser in an inscription mentions the slaying of Hoshea by his fellow Israelites.[11] The inference here is that the people, seeing the inevitable outcome of the contest with Assyria, put out of the way their fighting king, and then yielded submission to the conqueror, Tiglath-pileser III.[7] He is supposed by some to have been the "shepherd" mentioned in Zechariah 11:16.

And what can we conclude from this review of the life of Rezin, as compared with that of Arzu the Syrian. They have the same name. They have the same warlike temperament. They were both Syrian chieftains. And they both rose to power in the aftermath of some unnamed catastrophic event, the one described in the book of Amos, the other in the Harris papyrus. Is it merely a coincidence that the cause of those catastrophic event can be equated, and dated to the year 765 BC by the book of Amos, and to that same year by approximately 900 pages of argument in the first three books of our *Displaced Dynasties* series?

6) The papyrus describing the travels of an Egyptian dignitary named Wenamon proves conclusively that the reign of the 20th dynasty king Ramses XI and Herihor the founder of the 21st Theban dynasty must be dated in the approximate year 666 BC, thus confirming the essential accuracy of our Figure 1 timelines for these two dynasties.

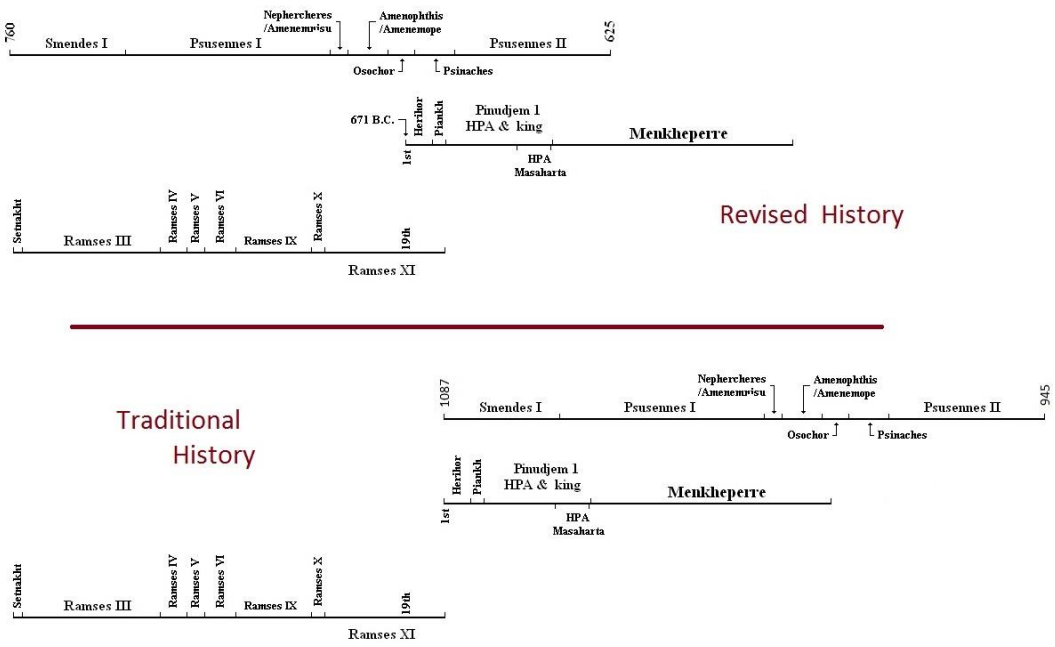
We will not spend more than a paragraph describing the mission of the Egyptian official known as Wenamon. His exploits are legendary, and a more than adequate summary is provided by the online Wikipedia site entitled "The Story of Wenamon". We strongly suggest that this article be read in its entirety, in spite of the fact that we disagree with much of it. An annotated summary of The Adventures of Wenamon is also available online, courtesy of R.A.S. Macalister, published in 1913 as part of his book entitled *The Philistines*. To read the entire story, we direct the reader to pages 25-29 of Pritchard's Ancient Near Eastern Texts (ANET), though it is also available on pages 306-13 of Gardiner's *Egypt of the Pharaohs*, for those who have access to that book. We are here interested in only a few details from the story, best summarized by ourselves.

All that the reader needs to know is the fact that Wenamon is commissioned by Herihor, the founder of the 21st Theban dynasty, to travel to Phoenicia and purchase timber to build a boat to transport the image of the god Amon, resident in Thebes. He sets out in the year 5 of an undefined era and travels north to Tanis in the eastern Delta, where he has been directed to obtain funds for the purchase from another dignitary named Nesubanebdjed. He then travels to Phoenicia where he is immediately robbed of those funds, and seeks redress from a king of Dor named Beder and later from Zakar-baal, aka Tjikar Ba'al, king of Byblos. In the course of the conversation with Zakar-baal he is informed concerning the fate of emissaries sent to Phoenicia years earlier by an Egyptian king named Khaemwaset, in context here the prenomen of Ramses XI. These emissaries had been unable to return to Egypt for unknown reasons, but the pronouncement clearly implied that there had been some turmoil in Egypt which precluded their return, and after years of enforced exile they had died in Phoenicia. Though the story continues, our interest in the story does not. So here we stop.

Though many scholars today treat the papyrus story as historical fiction it was certainly not disparaged as such in the aftermath of its purchase in 1891 by Golénischeff. Early on it became a foundational document in support of the traditional Egyptian chronology. The reference to Ramses XI, clearly no longer governing Thebes, informed critics that Wenamon's journey followed his death, possibly taking place only months after. Thus the journey of Wenamon is typically dated at the juncture between the 20th, and 21st Theban dynasty, 1087 BC in the opinion of Alan Gardiner (EP 446). And since, in the opinion of Egyptologists, the 21st Tanite and 21st Theban began and ended at the same time, the contemporary of Herihor in the Delta must be Smendes I the founder of the Tanite dynasty. It follows from the Wenamon story therefore, that Nesubanebdjed and Smendes I must be the same person. Since the publication of the Golénischeff papyrus in 1891 it has been axiomatic among Egyptologists that Smendes I has the nomen (or perhaps the prenomen) Nesubanebdjed, this in spite of the fact that no other Egyptian document corroborates that fact. And this faulty attribution is alone responsible for maintaining the fiction that the 21st Tanite and 21st Theban dynasties began at the same time. As the reader can see from our Figure I, we think otherwise, and deny categorically that Nesubanebdjed is one of the cartouche names Smendes I.

Following the adage that a picture is worth a thousand words we produce below a diagram featuring the timelines of the 20th, 21st Tanite, and 21st Theban dynasties, as understood by both the traditional history and the revised history. Clearly there are only two major differences. One is the positioning of the 21st Tanite dynasty in relation to the other two. The other is the sequence of absolute dates assigned to each. If Smendes I was a contemporary of Herihor the traditional history must be accepted. If Smendes I was a contemporary of Setnakht the revised history is correct. Alternatively, we can decide the issue between the two versions based on the date of the travels of Wenamon. If he is dated in the early years of the 11th century BC then the traditional history must be accepted. If instead he is dated in the 4th decade of the 7th century BC (or thereabouts) our revised history is validated. We decide here to focus on dates.

Figure 2: Relative positioning of the 20th, 21st Tanite and 21st Theban dynasties I in the revised versus the traditional history.



Before we begin we need to discuss one temporal feature of the Wenamon papyrus we have ignored thus far in this paper, but discussed at length in chapter 8 of our second book, namely, the mention by Wenamon that his journey began in the 5th year of some undefined era. This era is referenced in multiple hieroglyphic texts by the phrase *whm mswt*, and is acknowledged to have begun

in the 19th year of Ramses XI. If indeed Wenamon's journey began in the 5th year of that era (almost a certainty) then we can be more specific about the date of that journey. In Gardiner's opinion the reign of Ramses XI should be dated 1114-1087, his 19th year 1096 BC, and the journey of Wenamon 1092 BC. In our revised history Ramses XI is assigned dates 689-662 BC, his 19th year 671 BC, and the journey of Wenamon 667 BC.

In the traditional history there is absolutely no secondary evidence attesting the fact that the journey took place in 1092 BC or thereabouts. He is not mentioned in any other Egyptian document (other than a second version of the Wenamon story). There is no confirmation that a king Nesubanebdjed governed the eastern delta from Tanis at the time of his journey. There is nothing in the history of the times that remotely explains the mysterious era called the whm mswt. We have left the ordinal number "19th" suspended above the reign of Ramses in our Figure 2, but its meaning is a complete mystery. And there is certainly no evidence corroborating any details of the Phoenician context, other than the fact that the journey does assume a fairly recent arrival of the Tjekker tribe of Sea Peoples, a fact also provided by our revised history. There is certainly confirmation in this story that the 21st Theban founder Herihor governed the south of Egypt at the end of the 20th dynasty, but again, the revised history agrees totally. We repeat – there exists no evidence that the journey of Wenamon took place around the year 1092 BC. This absence of evidence is, of course, not evidence of Wenamon's absence. But it is certainly part of the reason why a large percentage of scholars treat the story as historical fiction.

The revised history fares much better, in large measure because it provides a clear and decisive rationale and dating of the *whm mswt*. In the revised history, as stated, the 19th year of Ramses XI is firmly identified as year 671 BC. That was the year the neo-Assyrian king Esarhaddon successfully overran the land of Egypt and began an Assyrian suzerainty that last till the year 637 BC, when the Assyrians lost control of Egypt to Menkheppere Piankhi. In 671 BC Esarhaddon took Ramses XI captive and exiled him from the country, confined to the western desert, where he remained till his death nine years later (662 BC). Meanwhile Egyptian documents referenced his years in exile as the whm mswt, a tacit admission that while Ramses' "reign" continued, he was no longer *de facto* in control of Egypt. A

good portion of our 8th chapter in book two discusses the issue. We will not repeat the evidence here.

As for the name of Wenamon we are equally fortunate. Esarhaddon died in 669 BC and was replaced by his son Ashurbanipal, whose reign in Assyria lasted over 40 years. Following the lead of his father, Ashurbanipal was an absentee landlord. According to his annals, dating from his 1st campaign (668/67 BC), he left the country in the control of approximately twenty nomarchs/proxy governors, each of whom is referred to as a king of their respective nomes, though in the same annals' listing they are referenced by their actual titles as "kings, governors and regents". The list is instructive, and certainly relevant to our argument considering the date, only a year or two prior to Wenamon's journey. We reproduce the list as translated on p. 294 of Pritchard's *Ancient Near Eastern Texts*:

> Necho (Ni-ku-u), king of Memphis and Sais (Sa-a-a), e Sharruludari, king of Si'nu, Pishanhuru, king of Nathu, Pakruru, king of (Pi)shaptu, Bukkunanni'pi, king of Athribis (Ha-at-hi-ri-bi), Nahke, king of Hininshi, Putubishti, king of Tanis (&2-nu), Unamunu, king of Nathu, Harsiaeshu, king of Sabnuti, Buaima, king of Pitinti, Shishak (Su-si-in-qu), king of Busiris (Bu-li-ru), Tabnahti, king of Punubu, Bukkananni'pi king of Ahni, Iptihardeshu, king of Pihattihurunpi(ki), Nahtihuruansini, king of Pishabdi'a, Bukurninip, king of Pahnuti, Siha, king of Siut (Si-ia-a-ti-tn), Lamentu, king of Himuni (Hermopolis), Ishpimatu, king of Taini, Mantimanhe, king of Thebes ; these kings, governors and regents whom my own father had appointed in Egypt and who had left their offices in the face of the uprising of Tirhakah [Tarqu] and had scattered into the open country, I reinstalled in their offices and in their (former) seats of office. (Thus) I seized anew (control over) Egypt (Musur) and Nubia which (already) my own father had conquered; I made the garrisons stronger than before and the(ir) regulations (more) severe . With many prisoners and heavy booty I returned safely to Nineveh.

This list is extremely instructive, though we will limit our discussion here to a few names. Most significant is the mention of "Unamunu, king of Nathu", almost certainly a reference to Wenamun. The name is right; the time is perfect. Unamunu clearly had the regulatory authority to act as a proxy on behalf of the Assyrian government on a diplomatic mission, not to speak of the freedom of movement required to leave on an extensive tour of duty elsewhere. It is perhaps significant that "Nathu" was the only region mentioned in Ashurbanipal's list that had two "kings", Unamunu and Pishanhuru, thus freeing Wenamon to spend a year or two on his protracted journey.

Elsewhere on this list are several other names clearly identified on our Figure 1 chart. Su-si-in-au (Sheshonk), king of Busiris, we have already discussed. He is the Hedjkheperre Sheshonk of the 22nd dynasty whose reign between Sheshonk III and Pemay is described in detail in Appendix 2 of our second book. We will devote a large section of the following paper to his reign. Interestingly, Pemay, his eventual successor, was assigned by Esarhaddon to govern the nome of "Pitinti", almost certainly a prestigious appointment leading to his eventual replacement of Sheshonk as the paramount leader of the 22nd dynasty. This identification, of course, is hypothetical, though the name is certainly right. The Akkadian transcribed as "Buaima" in ANET could also be transcribed "Puaima", and therefore, as was the case with Unamunu, the right name and right time (and even the right association with Sheshonk) strongly suggest that our identification is right. And for good measure we note the presence of the name of "Putubishti, king of Tanis" who may well be identified as our Pedubast I, a 23rd dynasty king, or even Pedubast II, not included in our Figure 1, dated by Kenneth Kitchen to the time of Ashurbanipal. We might add here for the record that we are almost certain that Pedubast I and Pedubast II are the same king. Again the name and the time argue for the identification. Though Pedubast was here installed as a regent in Tanis, there is nothing in Ashurbanipal's list to suggest that he was actually a pharaoh of the 21st dynasty, resident in that city. Instead he was appointed by Ashurbanipal to assist in regulating the affairs of the eastern Delta from a base in Tanis, apparently in conjunction with the 21st dynasty king Psinaches, whose duties as the resident high priest of Amon in Tanis were predominantly religious.

Our claim that Tanis had two rulers is not without precedent. We have already noted that Wenamon and Pishanhuru jointly ruled in Nathu. And in a moment we will see that Herihor and Piankh jointly ruled in Thebes. And of the two rulers in Tanis, Putubishti and Psinaches, we will have more to say. The reason is obvious. We have yet to discuss the true identity of Nesubanebdjed. And we have yet to identify the one name on Ashurbanipal's list that appears to be at odds with our revised chronology. We refer of course to "Mantimanhe, king of Thebes". Why do the names of Herihor and/or Piankh not appear as rulers in Thebes? And equally, why does Nesubanebdjed, a prominent figure in Wenamon's travelogue, not appear on Ashurbanipal's list? We have an answer to both questions. We will

be brief, considering the fact that we have already provided those answers in chapter 8 of book two, to which we have already provided a link.

First Mantimanhe. Already we have seen how Ashurbanipal's scribes have failed to articulate the true sounding of the Egyptian names. Either that or Assyriologists have misconstrued, or incorrectly ordered, the transcription of some of the cuneiform signs used in the representation of these names. Thus Pemay appears a Buaima and (Hedjkheperre) Sheshonk appears as Susinku. Worse still a king/regent of Hermopolis named Nimlot, appears as "Lamentu, king of Himuni". It should come as no surprise therefore, that Mantimanhe can be, and has been, otherwise transcribed as a reference to Piankhi, by no less an authority than the noted Egyptologist Flinders Petrie, on page 291 of volume three of his *History of Egypt*. Petrie was a firm advocate of the traditional history, and as such assigned the reign of Piankhi, the assumed founder of the 25th dynasty, to the middle years of the 8th century BC. Therefore, when he found evidence that a king by the name of Piankhi lived in days of Ashurbanipal, he simply took the evidence at face value and assumed, correctly as it turns out, that there were two Nubian kings that bore that name. His 2nd king Piankhi followed the first Piankhi by almost a century. Ours preceeds Menkheperre Piankhi by forty years. Regardless, Petrie was correct in his interpretation of the Assyrian annals reference, interpreting Mantimanhe king of Niu (Egyptian No = Thebes) as a reference to Piankh, the eventual successor of Herihor. We reproduce below his few remarks on the subject.

B.C. 702–670?] PANKHY II 291

The name of the ruler of Nia, Thebes, in the annals of the first expedition of Ashurbanipal, 668 B.C., is Manti·me·ankhe or Manti·pi·ankhi. It has been supposed to be Mentuemhat, but the ending *ankhi* cannot be intended for *hat*; the uncertain middle sign is therefore probably *pi*, and the reading is "Mer nuti Piankhi." If this is Pankhy II., and the bandage is accepted as reading 40 years or more, it would imply his ruling at 708 B.C. or earlier. This would not be at all impossible for his Ethiopian rule.

Petrie had unique access to the original clay tablets on which the Assyrian annals were recorded. If he says the sign for "me" was damaged and could well be a "pi"

and that the balance of the name should be read an-ki or an-khi, who are we to raise an objection. Needless to say his fellow Egyptologists objected, but not because of his reading of the cuneiform text. According to the traditional history the 26th Saite dynasty should be in its formative stage in the year 668 BC. The presence of a dignitary name Mentuemhet is demanded here if the traditional history is reliable. No wonder scholars dismissed Petrie out of hand, in spite of his admitted expertise. In our next paper we will again see Petrie at odds with his fellows.

Thus far we have explained the names on Ashurbanipal's list and clearly identified Wenamon as a prefect/king of Nathu. We have also hinted at our explanation for the absence of the names of Herihor and Nesubanbdjed on Ashurbanipal's list. They were high priests, not administrators. Piankh and Pedubast I were chosen instead for a governmental role. But if Herihor the high priest in Thebes sent Wenamon on a journey to receive money from his counterpart Nesubanebdjed the high priest in Tanis, it must be the case that Nesubanebdjed was the nomen or prenomen of the high priest ruling in Tanis in the year 768 BC. And that man would be Psinaches, the name Manetho provides for the king following Osochor and preceding a king Psusennes. Is there any evidence that the predecessor of Psusennes was named Nesubanebdjed?

If indeed Nesubanebdjed preceded Psusennes, then there is a strong possibility that he is the father of Psusennes. When we researched our second book we were able to find only one document suggestive of the fact that this father/son relationship did in fact exist. To understand the reference we need to remind the reader, for the umpteenth time, that traditional historians have confused the identities of the 21st dynasty kings Psusennes I and II. They have argued that the first Psusennes had the prenomen Aakheperre and the second the prenomen Tyetkheperre. We believe that Tyetkheperre ruled before Aakheperre. We will argue the case yet again in the next paper.

So Psusennes II in our Figure 1 is Aakheperre, sometimes referred to as the silver pharaoh, owing to the richness of his funerary artifacts and especially his splendid gold plated silver burial mask. And on p. 311 of book two we state:

> When Montet excavated the tomb of Aakheperre Psusennes (Psusennes I in the traditional history, Psusennes II in the revised alternative) he found on the body of

> Psusennes a pair of bracelets which contained inscriptions naming a "high priest of Amon Nesbanebdjed son of Menkheperre"

On p. 312 we add the fact that:

> The bracelet inscriptions contain more than simply the name and title of "Nesubanebdjed, son of Menkheperre". They contain, in parallel bands, the name of the HPA/king Psusennes himself, and the phrase “made by (ir.n) the HPA Nesubanebdjed, son of Menkheperre. The complete inscription, as translated by Montet, reads instead "Psusennes, born of the high priest Nesbanebdjed, son of Menkhepperre". Montet was clearly influenced in his translation by the fact that Manetho lists Psusennes I as a son of a king Smendes I, considered by the traditional history to be a king named Nesubanebdjed. Thus his unusual translation of the Egyptian hieroglyphs “ir.n” and his identification of the Nesubanebdjed on the bracelets.

Now we might have stopped our analysis at this point and accepted Montet’s translation of the bracelet inscription. If “ir.n” means “born of” then Nesubanebdjed *could be* the nomen of Psinaches, the father of Aakheperre. We say “*could be*” because we cannot prove that a father/son relationship did exist between Psinaches and Aakheperre, and we have yet to prove absolutely that we are correct in identifying Aakheperre as the 2nd king Psusennes. As it stands the traditional history is quite comfortable with Montet’s mistranslation. They firmly believe that their Smendes I had the nomen Nesubanebdjed and that he was the predecessor of Aakheperre, and could well have been his father. So we continued, especially since “ir.n” on funerary attire always means “made by”.

But continuing got us nowhere. At best we could surmise that “the Nesubanebdjed named on the bracelets must be an intimate family member, and might be Psusennes’ father, but for identical reasons cited in the previous paragraph, we could not distinguish between our claim that the father of Aakheperre was Nesubanebdjed/Psinaches and the claim by traditionalists that the father of Aakheperre was Nesubanebdjed/Smendes I. We did briefly discuss the possibility that the funerary bracelets might have been made by a high priest named Nesubanebdjed [aka Smendes (II) in the textbooks], who was a son of the 21st Theban priest/king Menkheperre [whom we identify as the 25th dynasty king Piankhi] but rejected that for a variety of reasons, not relevant here. In hindsight, we now believe that the latter identification was correct. And for what it’s worth, traditional historians also accept this interpretation of the bandage inscriptions.

We reference the funerary bracelets of Aakheperre for two reasons. **Our previous discussion clearly demonstrates a need for us to positively identify Aakheperre as the Psusennes II in our Figure 1. We hope to do that in the near future.** And assuming we can do that, we will be in a perfect position to positively identify Psinaches. The reasoning is straightforward. If the traditional history argues strongly that the predecessor of Aakheperre was a king named Hedjkheperre setepenre Nesubanebdjed meryamun (whom they identify as Smendes I) and we argue strongly that the predecessor of Aakheperre was a king named Psinaches, and that Psinaches had the nomen Nesubanebdjed, **it is extremely likely that Psinaches is the king whose cartouche names are Hedjkheperre setepenre Nesubanebdjed Meryamun. We need to argue that case as well in the near future.** In fact, we will begin our next paper by establishing both facts.

Thus far we have answered almost every conceivable relevant question regarding the names on Ashurbanipal's roster of "kings, governors, and regents." We are informed by that list that the following dignitaries were alive and well in the year 668/7 BC, all named on our Figure 1 - Piankh & Pedubast, Ashurbanipal's appointees assisting the religious authorities Herihor and Nesubanebdjed in Thebes and Tanis respectively; Hedjkheperre Sheshonk, author of the Bubastite portal inscription (and certainly not Sheshonk I founder of the 22nd dynasty), and Pemay, his successor; and finally Wenamon himself. Combine that with our explanation of the *whm mswt* and we are satisfied that we have sufficiently defended our dating of the escapades of Wenamon.

One bookkeeper task remains. The story of Wenamon featured a long dialogue between himself and the "prince of Byblos" named Zakar-baal. The date was probably early in the year 666 BC. We need to add this name to our list of kings of Byblos created in our paper #3, assuming he was a king, not a prince. We will have more to say about him at a later time. If the reader questions why the last three kings in our list are assigned such short reigns we can no better than suggest that the early 7th century consisted of one traumatic episode after another for kings on the eastern coast of the Mediterranean, subject to repeated raids by the neo-Assyrian kings, and forced labor in their service. We also suggest the possibility, absolutely consistent with all the evidence, that Abibaal, Milki-

ashapa, and Zakar-baal were brothers, all sons of Yahawmelek, who himself became king at an old age. Time will tell if we are right.

Table 1: Ten 9th-7th century kings of Byblos

King of Byblos	Approximate regnal years BC	Approximate date of source BC
Ahiram	820-800	n/a
Ithbaal	800-780	800 (inscription)
Yehimelek	780-760	760 (inscription)
Elibaal	760-740	750 (inscription)
Shipitbaal	740-720	740 (Tiglath-Pileser III annals) 730 (inscription)
Urimelek	720-700	701 (Sennacherib annals)
Yehawmelek	700- 680	700 (inscription)
Abibaal	680-672	674 (inscription)
Milki-ashapa	672-667	670 (Esarhaddon annals) 668 (Ashurbanipal annals)
Zakar-baal	667-650	666 Wenamun papyrus

Synopsis: Our primary objective for this paper - verifying the relative positioning and absolute dating of the five Egyptian timelines in our Figure I - has been met. And we will repeat the process again in the next paper. Thus far …

1. We have confirmed the fact that our dating of the Santorini explosion is plausible, and certainly consistent with the archaeological record and radiocarbon results.

2. We have explained the near simultaneous emergence of the 20th, 21st Tanite, 22nd & 23rd Libyan dynasties.

3. We have established the fact of intermarriage between the royal families of the 20th, 21st Tanite, and 22nd dynasty kings, suggesting, if it doesn't prove, that they were contemporaries.

4. We have attested, via the annals of the neo-Assyrian kings Sennacherib, Esarhaddon & Ashurbanipal, the existence of multiple concurrent dynasties in

Egypt, with the 23rd dynasty kings named Takeloth (Assyrian Tarqu) located in the extreme south of the country, in this era a region called Melukkha.

5. We have verified**,** thanks to the annals of Tiglath-Pileser III and the Hebrew Bible, the presence of, and continuing aggression of, the Syrian renegade king Irsu (Rezin) in the 3rd quarter of the 8th century, confirming our dating of Ramses III.

and

6. We have authenticated**,** via the annals of Ashurbanipal, the 667 BC dating of Wenamun, a 21st Tanite dynasty diplomat/regent, and we have synchronized his famous journey with kings from the 20th, 21st Theban, 22nd and 23rd dynasties, who occupied the identical time frame, thus confirming the dating of all five Egyptian dynasties in our Figure 1.

Convincing? Let the reader decide.

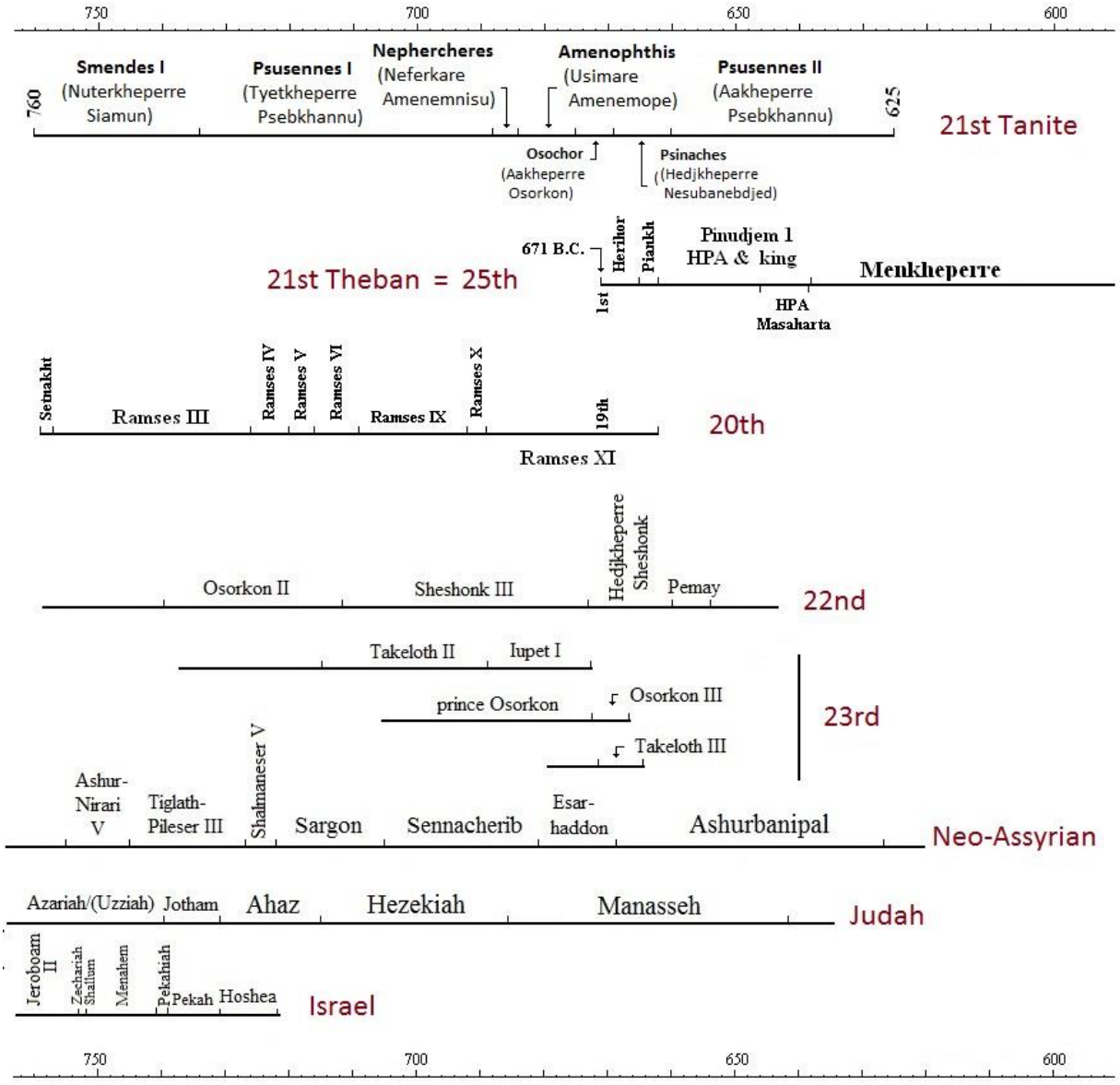
Paper #6 The fact that Egypt was governed by five overlapping dynasties in the century following the Santorini mega-explosion (ca 760-650 BC) is confirmed by the Dibabiyeh stele, the Petrie version of Pasenhor's genealogy, the Berlin & Louvre stelae, and a fragmentary relief on a building south of the main temple of Amun in Tanis.

In our previous paper we began by producing a timeline chart depicting our revised positioning of Egyptian dynasties 20, 21 Tanite, 21 Theban, 22 and 23, alongside the established chronologies of the Neo-Assyrian kings and the kings of Israel and Judah from the Hebrew Bible, all in the 760-650 BC time frame. The primary objective then was identical to that of this paper, i.e. to confirm that those timelines are correct in every respect. We begin by duplicating that timeline chart as our Figure 1, but only after making a few cosmetic changes.

The changes we refer to are related to a secondary aim of this sequence of papers. We want to provide actual cartouche names for all of the pharaohs named on that chart, beginning with those listed on our 21st Tanite dynasty timeline. This objective is largely motivated by comments we made at the conclusion of our previous paper, where we repeated our frequently voiced opinion that the two 21st dynasty kings named Psusennes have been confused by traditional history scholars, and that the cartouche names Hedjkheperre Nesubanebdjed, linked to Smendes I by those same traditional historians, actually belong to Manetho's king Psinaches. Thus we begin this paper by including the correct cartouche names for Psusennes I & 2, and for Psinaches. And since in the course of this essay we will supply Smendes with his actual cartouche names, and reveal the true identity of Manetho's king Osochor, we include those names as well in our Figure 1. Proof will follow.

As in paper #5 we provide here a sequence of stand-alone points. The paper is longer than usual (over 30 pages), but is well worth a carefully reading. If subject to time-constraints the reader may find it convenient to spread the task out over time, allowing opportunity to evaluate the argument and read the links to online sources. In the last paper we produced six independent articles, each focussed on the same objective. In this paper, due to the complexity of the arguments, there are only five, labelled A to E below.

Figure 1: Revised history timelines of the kings of Israel, Judah, Assyria, and Egypt in the approximate time frame 750-650 BC, with the inclusion of cartouche names for all seven 21st Tanite dynasty kings.



A. Identifying our Figure 1 king Psusennes II as a king named Aakheperre Psebkhannu and our Figure 1 king Psinaches as a king named Hedjkheperre Nesubanebdjed, verifies the accuracy of our positioning and absolute dating of all of the dynasties in that timeline chart.

We begin this discussion by clarifying a feature of the Figure 1 chart that we ought to have explained previously, but didn't, this because our four books on the subject of Egyptian chronology were directed toward Egyptologists and others well acquainted with Egyptian history. Those readers were already aware of the fact that all of the names on our 21st Tanite timeline were derived from Manetho, via excerpts copied by Josephus and by later Greek speaking scholars such as Africanus and Eusebius. A few of Manetho's names do actually resemble those possessed by known pharaohs, but at least three key names are not so clearly identified. Thus there exist absolutely no Egyptian inscriptions containing the names Smendes, Psusennes, and Psinaches, as written by Manetho, a regrettable absence since those are precisely the names we are most interested in in this paper.

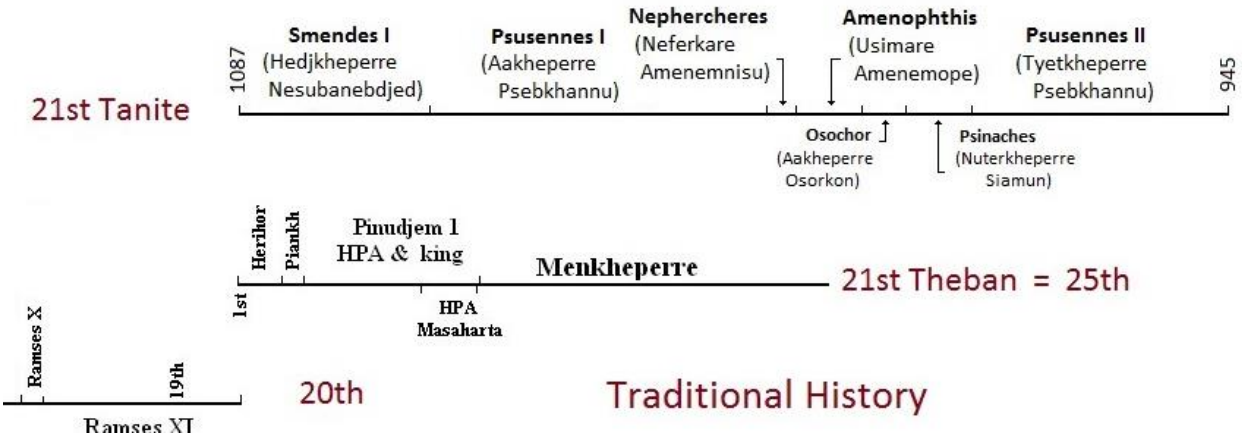
Consequently Egyptologists, following the decipherment of the hieroglyphic script in the 3rd decade of the 19th century, had to determine from a limited number of resource documents which pharaohs should be associated with which of Manetho's Greek names. They did their best, early on determining that two kings bearing the "birth name"/nomen Psebkhannu (or Pasebkhannu) must be identified with Manetho's two kings named Psusennes. But they guessed wrong on which Psebkhannu should be identified with which Psusennes. They also chose the wrong king to identify as Manetho's king Smendes. We have commented frequently on the cause of the latter mistake. Here we review the details again.

The story of Wenamon's travels, discussed at the end of our previous paper, was largely the source of the mistaken opinion. That story clearly identified the fact that Ramses XI, the terminal king of the 20th dynasty was succeeded by Herihor, the initial king of the 21st Theban dynasty. It also identified as Herihor's contemporary in Tanis an individual named Nesubanebdjed. And since traditional historians already believed, entirely without evidence, that the 21st Tanite

dynasty in the north of Egypt, along with the 21st Theban dynasty in the south, together replaced Ramses XI in governing the whole of Egypt, it followed that Nesubanebdjed must be the nomen of Smendes, identified by Manetho as the founder of the 21st dynasty. And finally, since there existed then, and there still exists today, only one Egyptian king with the nomen Nesubanebdjed, this king was the unanimous choice as the 21st Tanite dynasty founder. His name – Hedjkheperre Nesubanebdjed.

Thus, from the middle years of the 18th century till today, Egyptian chronology has been cemented in place as diagrammed in Figure 2 below. Argument still exists on the absolute dates assigned each king, but the relative positioning has remained unchanged, with one possible exception, noted later in this paper.

Figure 2: Traditional Egyptian chronology of the 21st Theban, and 21st Tanite dynasties in relation to the death of Ramses XI.

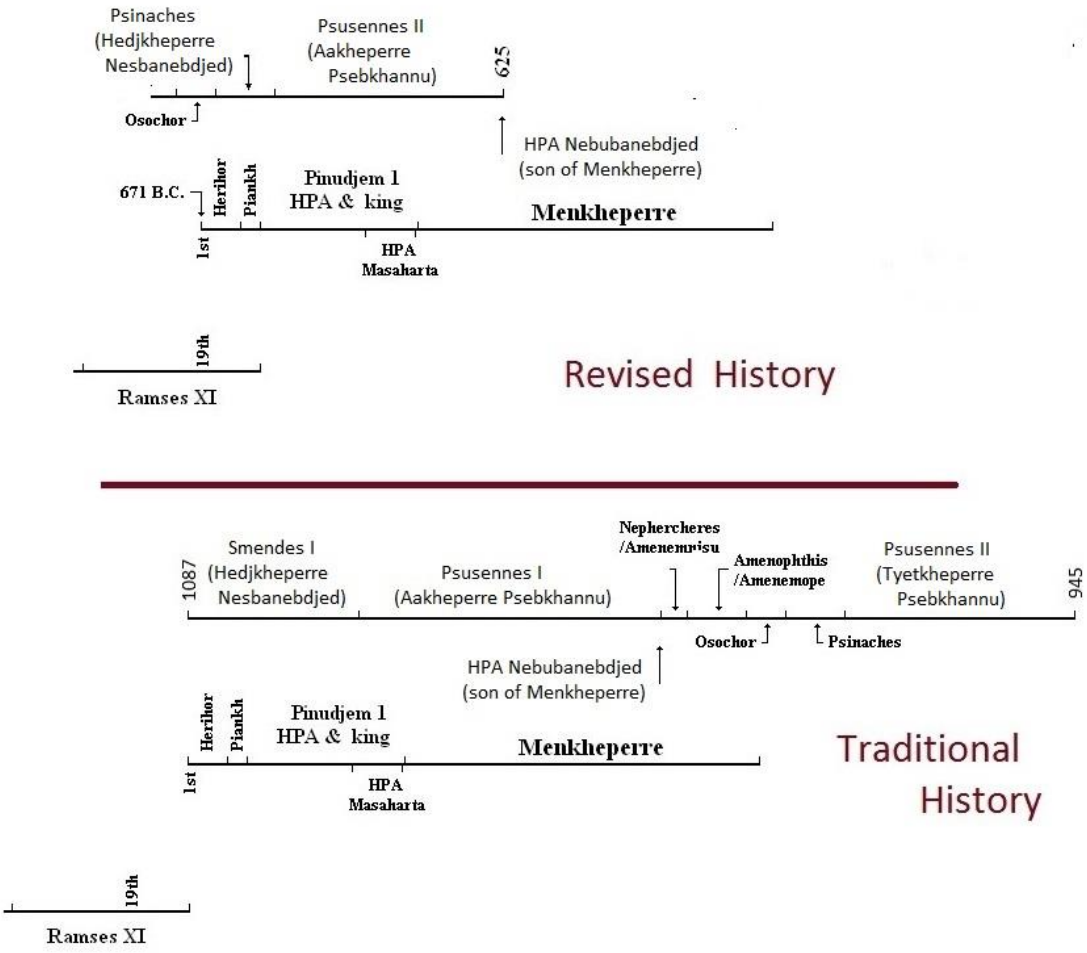


Our revised chronology of these three dynasties, as diagrammed in our Figure 1, differs from the traditional history in three respects. One is the absolute dates assigned these pharaohs. The second is the fact that we have switched the identities of the kings Psusennes I & II, and assigned the name Hedjkheperre Nesubanebdjed to Psinaches, not Smendes. And finally, early on we recognized that Smendes, the 21st dynasty founder, was not a contemporary of Herihor and Ramses XI, but was instead a contemporary of Setnakht, the founder of the 20th

dynasty. Thus the 21st Tanite dynasty timeline in Figure 2 had to be displaced approximately 100 years to the left, 100 years being the approximate length of the 20th dynasty. This displacement brought the 21st dynasty kings Osochor and Psinaches to a position contemporary with Herihor, where they appear in our Figure 1.

It is time to defend the changes we have made to our Figure 1. In this first section of our paper our arguments all relate to the time frame from Herihor to Menkheperre, as represented by the timelines of both the traditional and revised histories. For convenience we reproduce those timelines as they appear in our Figures 1 and 2, duplicating them in a single chart, our Figure 3 below.

Figure 3: Traditional History and Revised History timelines describing Egyptian history following the death of Ramses XI.



At this time we make only four comments in defense of our "Revised History" as displayed in Figures 1 and 3.

1) The reader will notice that we have added to both sections of our Figure 3 an entry related to the bracelets found on the mummy of king Aakheperre Psebkhannu in his intact tomb in Tanis, as discussed at the end of our previous paper. Those bracelets named Aakheperre, and claimed they were "made by" a dignitary named "Nesubanebdjed, son of Menkheperre", undoubtedly a reference to the High Priest of Amon (HPA) named Nesubanebdjed, a son of the priest/king Menkheperre. At first we questioned whether the bracelets were indeed a gift from Menkheperre via his son, but in the end we accepted the identification, knowing full well that this attribution demanded that we identify Psusennes II as the king Aakheperre Psebkhannu. Those mummy bracelets made absolutely no sense in our Figures 1 & 3 before we made the name change, but fit perfectly the situation as now diagrammed. And while they make sense also in the traditional history timeline, our revised history is able to explain a fact for which the traditional history has no explanation, namely, why the priest king Menkheperre (alias Piankhi), early in his life, relinquished his titles as the High Priest of Amon to his son Nesubanebdjed. In our revised history we argued the case that in the year 637 BC Menkheperre led a rebellion that drove the Assyrians out of Egypt, and established himself as the *de facto* ruler of the whole of Egypt. Subsequently he relinquished his clerical titles to his son Nesubanebdjed, and later, when Aakheperre Psebkhannu died, he directed this son to make bracelets to adorn the body of the 21st dynasty king. And in a moment we will comment on the genealogical link between the 21st Tanite and 21st Theban (= 25th dynasty) families that would explain this act of devotion.

2) On pages 3 & 4 above we cited the travels of the Egyptian dignitary Wenamon as the primary evidence supporting the traditional history in their belief that Smendes I and Hedjkheperre Nesubanebdjed were one and the same person. But a glance at our Figure 3 should convince our readers that we can cite precisely the same arguments for identifying Hedjkheperre and Psinaches, thus removing the only deficiency in the argument we proposed at the end of the previous paper. And there we had the advantage of a mass of evidence from the

annals of the Assyrian king Ashurbanipal to support our argument. And since our dating of Herihor and Piankh and Psinaches, contemporaries of king Nesubanebdjed , was arrived at long before, and independent of, any consideration of the Wenamon papyrus, the timelines in Figures 1 and 3 absolutely confirm the equation Psinaches = Hedjkheperre Nesubanebdjed.

3) Speaking of timelines which confirm the fact that Psinaches must be identified as Hedjkheperre Nesubanebdjed, and that this king lived around the middle years of the 8th century BC, we need to examine one further document - the so-called Dibabiyeh/Dibabieh quarry stele. Along with the Wenamon story, the inscription on this stele is cited in the literature of the traditional history as one of the few mentions of king Hedjkheperre Nesubanebdjed in an actual historical context. Indeed the Wenamun papyrus and the Dibabieh stele are the only mentions of this king in a recognized historical context. How convenient therefore that we are able to cite them both as evidence supporting the equation Psinaches = Hedjkheperre Nesubanebdjed.

We introduce the Dibabieh stele by quoting Robert K. Ritner, who produces both a transcription and a translation of the entire stele inscription on pages 101-104 in the volume *Libyan Anarchy: Inscriptions from Egypt's Third Intermediate Period*, edited by Edward Wente. In his introductory note on page 101 Ritner describes the stele and its contents:

> A hieroglyphic stela carved on a pillar in the western gallery of the limestone quarries at Dibabieh, near the town of Gebelein, preserved a rare mention of Smendes in Upper Egypt. Below a winged disk, the stela was framed by vertical bands of text recording the titles of the king, although only the right band remained for modern copyists. In addorsed scenes above the primary inscription, Smendes was depicted worshiping Amon and Khonsu on the right and Amon and Mut on the left. The main text of seventeen lines adopts the standard *Königsnovelle* format, in which the king is informed of disturbance and promptly orders corrective action. Particularly noteworthy is the royal presence at Memphis (rather than at Tanis) and the destructive flooding of Luxor temple, encountered again during the tenure of Osorkon III. A broken passage in line 15 suggests a census of project workers, noting deaths and new births. The primary textual edition is in places unreliable, but the stela is now destroyed below the cornice and winged disk (personal communication, James A. Harrell, 19 October 1999).

According to Ritner's introductory note, the stele inscription is entirely concerned with a massive flood of the Nile, so large that it threatened the Luxor temple in Karnak, reminiscent of a flood that took place "during the tenure of Osorkon III".

Identifying the author of the inscription is not problematic. Multiple times Hedjkheperre Nesubanebdjed proudly displays both of his cartouche names, as in the following excerpt from the introductory portion on p. 102.

> nṯr nfr nb tȝ.wy Ḥḏ-ḫpr-Rʿ-stp-n-Rʿ sȝ Rʿ nb ḫʿ.w Ns-bȝ-(nb)-Ḏd.t mrî-ʾImn mȝ[…] Ḥr-ȝḫ.ty(?) […]n r ny-sw.t nb […] dî ʿnḫ ḏd wȝs nb snb nb mî Rʿ
>
> The Good God, Lord of the Two Lands, Hedjkheperre-setepenre, the Son of Re, Lord of Diadems, Smendes, beloved of Amon, […] Horachty (?) […] more than any king […], given all life, stability, and dominion and all health like Re.

We have included in this quoted paragraph the transliteration of the hieroglyphic text from which the translation derives, this for a reason. We want to advise our readers to be careful when reading Ritner's translation, because he never does actually transcribe the name of Nesubanebdjed when it occurs in the stele inscription. He merely follows the traditional party line, referring to Nesubanebdjed as Smendes in each and every instance where that name occurs, including his reference to Smendes in his introductory note, duplicated above.

On page 103 Ritner translates the critical section of the Dibabiyeh stele, that which documents the massive flood with which the inscription is solely concerned. Here we provide only his translation.

Now His Majesty was in the city of Memphis, his noble residence of valor and victory like Re [... He went (?) to the estate of Ptah,] (4) lord of Ankh-Tawy, and Sakhmet the great, the beloved of Ptah [...], Montu and the Ennead who are in the "Wall of the Sovereign."[2]

Now His Majesty was seated in the columned [hall, and one came to say to] (5) His Majesty: "The canal bed[3] that formed the borders of Luxor temple and that was made by King Menkheperre (Thutmosis III) is fallen [into ruin. ...] (6) There is a great flood and a strong current with[in] it on the great pavement of the temple. It has encircled the front [...]."

[Now His Majesty said] (7) to them: "If it is a matter said before me, yet it is not something at all in the experience[4] of My Majesty, anything similar being unknown [...] (8) protect from the calamity[5] there. It was a canal harnessed seasonally for the borders of [...] (9) in ignorance of my property (?), all these being remote from the sovereign."

Then His Majesty [sent architects] (10) and 3,000 men with them, comprising the best of the comrades of His Majesty. To them His Majesty commanded: "Go to [the south (?) ...] (11) desert [...] the commanders of His Majesty as subordinates at his heels to create heaps (of stones) in [the quarry (?) ...] (12) witness [...] in the vicinity of this[6] quarry from the time of those who have passed away until today, Gebelein [...] (13) the temple of Montu, lord of Tod. They engraved this decree that causes that His Majesty remain [...] (14) excavated[7] by themselves monthly. His command arrived to sanctify the works of the decree [...] (15) in reckoning those who had passed away and the child at the breast of his mother up to [... Never was anything] (16) done similarly in the time of the ancestors.

And what is the point we are attempting to make in the above documentation. Perhaps the astute reader will already have guessed, particularly if he/she has our Figure 1 in view. When Ritner reads this stele inscription one notorious flood comes to his mind immediately, that which took place in the days of Osorkon III, a king dated by the traditional history to the first decade of the 8th century BC., thus roughly three hundred years after the reign of Smendes I, dated ca 1087-1061 BC by traditional scholars. But let us set the record straight. The flood described on the Dibabiyeh stele is not just reminiscent of one that took place in the time of Osorkon III*, it is Osorkon's flood*.

Nile floods were documented carefully by Egyptian officials, and Nile flood levels were carefully recorded on the walls of the quay at Karnak. Only two are of proportions comparable to what is described in the Dibabiyeh stele. The largest took place in the third year of the reign of Osorkon III, as noted by Ritner. The second largest took place in the 6th year of the 25th dynasty king Taharkah. We discuss both floods on pages 119-120 of our first book, where at the time we

made the mistake of trusting older Egyptologists, who assigned the earlier flood to the reign of Osorkon II, not III, an error corrected since the writing of our first book.

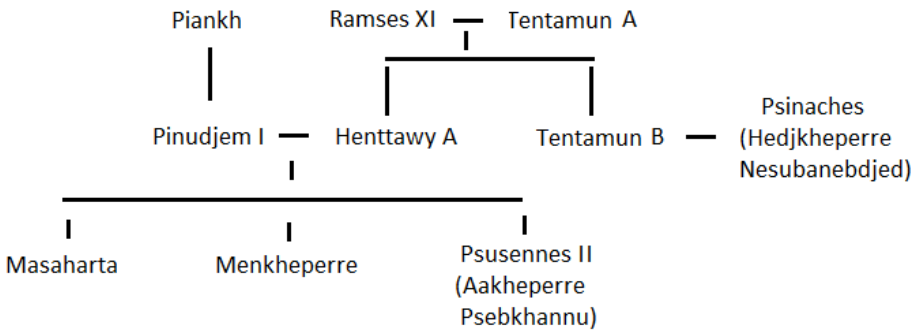
And why the length of the above preamble? A glance at our Figure 1 tells the tale. The reign of Psinaches overlaps the 3rd year of king Osorkon III, the known date of the massive Nile flood. Coincidence? We think not. The Dibabiyeh stele now becomes support for our identification of Manetho's Psinaches with Hedjkheperre Nesbanebdjed. By contrast, the traditional history is now bereft of any support for its association of the name Hedjkheperre Nesubanebdjed with Smendes I.

4) Our final defense for both the Hedjkheperre Nesubanebdjed and Psebkhannu name changes is actually a structural argument. One feature of our Figure 3 should immediately strike the reader, in this particular instance, as truly coincidental. With our name changes, and our shifting of the 21st Tanite dynasty backward in time a full century, the revised history and traditional history relative positioning of the main characters in our drama are almost exact duplicates, absolute dates excepted. In both histories two of the key figures involved in our name changes, Hedjkheperre Nesubanebdjed and Aakheperre Psebkhannu, end up overlapping completely the reigns of the 21st dynasty Theban clerics Herihor, Piankh, Pinudjem I, and Masaharta, and partially at least the tenure of the priest/king Menkheperre. It follows that the vast majority of relationships between the 20th, 21st Tanite, and 21st Theban dynasty kings in this limited time frame, as determined by Egyptologists based on extant inscriptions, are precisely those which our revised history would confirm and endorse from those same documents. And without exception Egyptologists have determined that all three dynasties are related by marriage, two daughters of Ramses XI featuring prominently in the resulting genealogies.

Egyptologists differ on some of the genealogical details, but are in general agreement with the overall schema. We follow here the version proposed by Kenneth Kitchen, who diagrams two variant interpretations of the available source documents in sect. 441 (p. 538) of his classic TIP (2nd edition with

supplement). We summarize the details common to both variants in our Figure 4 below. Almost all Egyptologists accept the basic facts summarized in that flow chart, with Pinudjem I marrying Henttawy A, a daughter of Ramses XI, and fathering the three future kings, Masaharta, Menkheperre and Aakheperre Psebkhannu, though some would argue that Masaharta was the father, not the brother of Menkheperre, and others that Masaharta and Menkheperre were children of Pinudjem I by a different wife than Henttawy A. Regardless, this genealogy provides a probable explanation for why Pinudjem's son Menkheperre would name his son *Nesubanebdjed* (after his maternal uncle), and why he would direct that son to make bracelets for the funeral of *Aakheperre Psebkhannu* (his brother or half-brother). And Kitchen's genealogy absolutely confirms the fact that Psusennes II in our revised history must be identified as the king Aakheperre Psebkhannu.

Figure 4: Key descendants of the 20th dynasty king Ramses XI, and the 21st Theban dynasty king Piankh, according to Kenneth Kitchen, (replacing Kitchen's names Smendes and Psusennes I with the revised history equivalents Psinaches and Psusennes II).



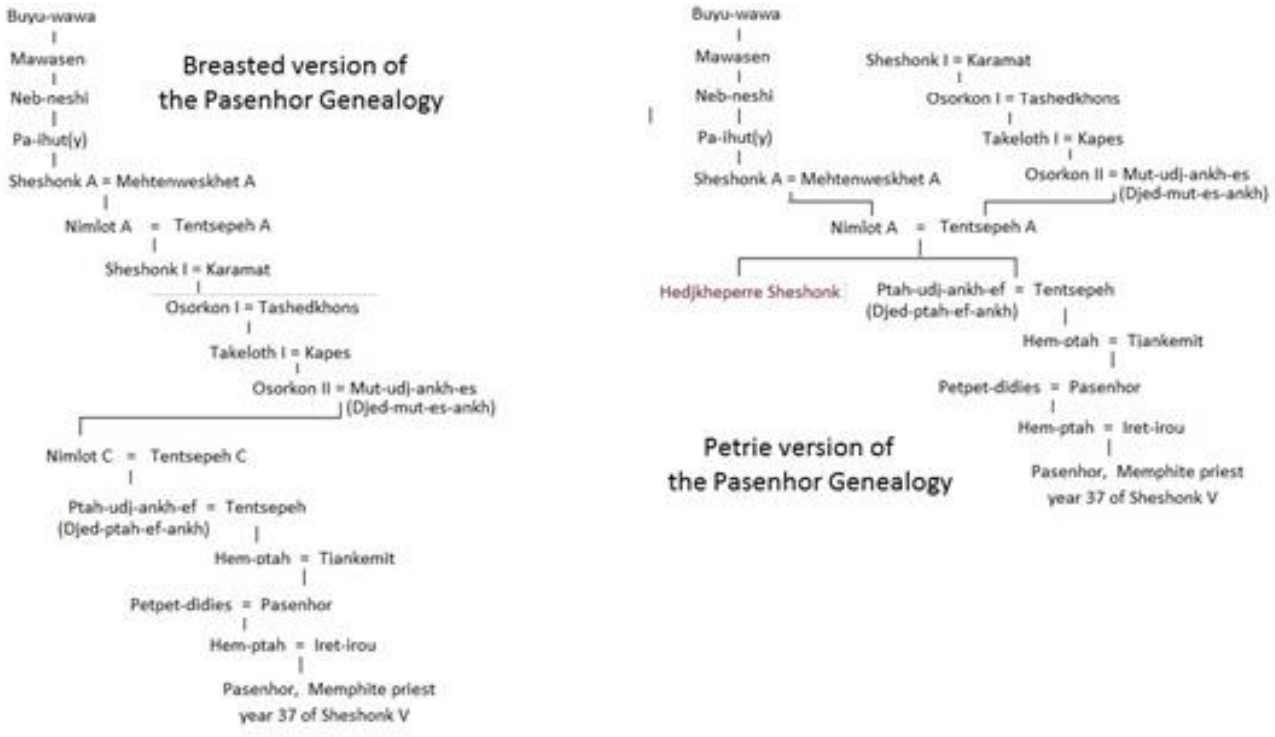
B. Adding "Osorkon the Elder" to the Flinders Petrie version of the Pasenhor genealogy confirms the relative positioning of our 21st and 22nd dynasty timelines in Figure 1.

In Appendix A of *Piankhi the Chameleon,* the second book in our *Displaced Dynasties Series*, we discussed the genealogy of a Memphite priest named Pasenhor, and diagrammed the two ways his genealogy could be interpreted. We rejected the interpretation of the well-respected Egyptologist James Henry

Breasted (the version now adhered to by almost every Egyptologist on earth) and we adopted the version approved by the equally highly respected Egyptologist Flinders Petrie (the version followed in the 20th century by Petrie and no one else). In the course of doing so we made one addition to Petrie's version, adding the name of a recently discovered pharaoh named Hedjkheperre Sheshonk as a son of Nimlot A and Mehtenweskhet A. And we spent the whole of Appendix B in our book two discussing this pharaoh, his discovery by the Egyptologist Aidan Dodson in 1993, and his approximate positioning in the 22nd dynasty between the reigns of Sheshonk III and Pemay. Since we will spend time in a moment discussing the Pasenhor genealogy, we highly recommend that the reader digest the contents of both Appendix A and Appendix B in book two.

In our Figure 5 below we reproduce from Appendix A both the Petrie and Breasted interpretations of the Pasenhor genealogy, copied respectively from Figures 25 and 21 in that Appendix.

Figure 5: The Breasted and Petrie versions of the Pasenhor genealogy (reproduced from Figures 21 and 25 of Appendix A in the book *Piankhi the Chameleon*)

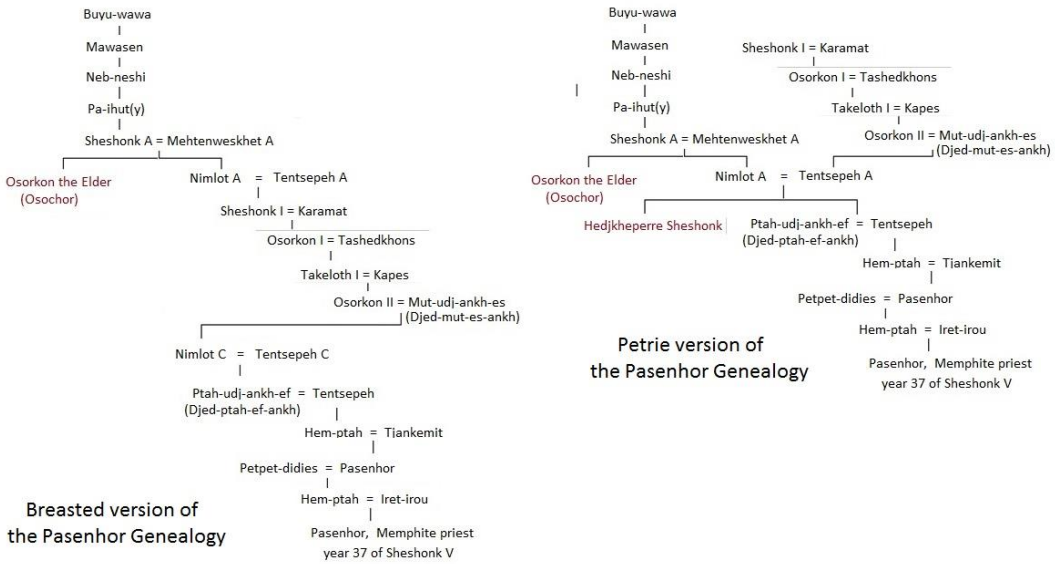


The reader should take note of one major difference between the two interpretive versions. In Breasted's opinion Nimlot A and Mehtenweskhet A feature as the parents of a son named Sheshonk (I), the reputed founder of the 22nd dynasty (hence the numeral I affixed to his name), and the claim is made that this son had the prenomen Hedjkheperre – a claim supported by absolutely no evidence. In Petrie's opinion Nimlot A and Mehtenweskhet A feature as the parents of an otherwise obscure son named Djed-ptah-ef-ankh, whose burial in the tomb DB320 is discussed in our Appendix B, and is mentioned again later in this paper. We have added a second son, Dodson's newly discovered pharaoh Hedjkheperre Sheshonk, in precisely the same position as Breasted's Sheshonk I, but in this case we are certain that this Sheshonk, son of Nimlot A, bore the prenomen Hedjkheppere, and we are informed by the genealogy that he is not Sheshonk I. We should point out, of course, that Petrie had no knowledge of the existence of a "second" Hedjkheperre Sheshonk, and like Breasted was of the opinion that the king identified as Sheshonk I in his version of Pasenhor's genealogy had the throne name Hedjkheperre.

We now want to make additional changes to Pasenhor's genealogy, this time to both the Breasted and the Petrie versions, not only to further validate our 22nd dynasty timeline in Figure 1, but the 21st dynasty timeline also. After all, confirming the accuracy of the timelines in that initial Figure 1 is the main purpose for which this paper is written.

We begin by adding a single name to both charts, namely, that of Osorkon the Elder, the 5th priest/king of the 21st dynasty, an individual known to Manetho by the name Osochor. We add the name in Figure 6 below as a second son of the parents Sheshonk A and Mehtenweskhet A .

Figure 6: Chart showing the contrasting Breasted and Petrie interpretations of the Pasenhor genealogy (with later additions in red).



The name of Osochor is already part of our 21st Tanite dynasty timeline in Figure 1. Precisely how a 22nd dynasty Libyan, son of non-royal parents, emerged as the 5th king of a 21st dynasty of priest/kings, is beyond the understanding of this author. Suffice to say, absolutely every Egyptologist today agrees with that placement of this king Osochor, as do we. We restrict our explanation here to that provided by the Wikipedia article on Osorkon the Elder, a portion of which is quoted below.

> Akheperre Setepenre Osorkon the Elder was the fifth king of the twenty-first dynasty of Egypt and **was the first pharaoh of Libyan extraction in Egypt**. He is also sometimes known as "Osochor," following Manetho's Aegyptiaca.
>
> Osorkon the Elder was the son of Shoshenq, the Great Chief of the Ma by the latter's wife 'Mehtenweskhet who is given the prestigious title of 'King's Mother' in a document. Osochor was the brother of Nimlot A, the Great Chief of the Ma, and Tentshepeh A the daughter of the Great Chief of the Ma and, thus, **an uncle of Shoshenq I, founder of the Twenty-second Dynasty.**
>
> **His existence was doubted by most scholars until Eric Young established in 1963 that the induction of a temple priest named Nespaneferhor in Year 2 I Shemu day 20 under a certain king named Akheperre Setepenre - in fragment 3B, line 1-3 of the Karnak**

> **Priest Annals - occurred one generation prior to the induction of Hori, Nespaneferhor's son in Year 17 of Siamun, which is also recorded in the same annals. Young argued that this king Akheperre Setepenre was the unknown Osochor.** This hypothesis was not fully accepted by all Egyptologists at that time, however.
>
> But in a 1976-1977 paper, Jean Yoyotte noted that a Libyan king named Osorkon was the son of Shoshenq A by the Lady Mehtenweshkhet, with Mehtenweshkhet being explicitly titled the "King's Mother" in a certain genealogical document. Since none of the other kings named Osorkon had a mother named Mehtenweshkhet, it was conclusively established that Akheperre Setepenre was indeed Manetho's Osochor, whose mother was Mehtenweshkhet. The Lady Mehtenweshet A was also the mother of Nimlot A, Great Chief of the Meshwesh and, thus**,** Shoshenq I's grandmother.

The reader needs to be cautioned not to accept every word in this article. We have emphasized three sections that need correction. "Osorkon the Elder … **was the first pharaoh of Libyan extraction in Egypt**" only if you accept the Breasted version of the Pasenhor genealogy. In the Petrie version he is preceded by Sheshonk I, Osorkon I, Takeloth I, and Osorkon II, as he is in our Figure 1. Likewise for the statement that Osochor was "**an uncle of Shoshenq I, the founder of the twenty-second dynasty**". He was instead an uncle of Dodson's second Hedjkheperre Sheshonk, who is definitely not Shoshenq I (see Figure 1). The second error can be quickly corrected if the reader simply deletes every mention of the name Sheshonk 1 in that article and substitutes the name Hedjkheperre Sheshonk. We have already multiple times argued our case, and will argue it again in a moment, that Sheshonk I, the founder of the 22nd dynasty, did not possess the prenomen Hedjkheperre. At minimum there is absolutely no evidence that he did.

A third error is the whole of the third quoted paragraph which discusses the discovery of Eric Young. Absolutely nothing in that paragraph concerns the king Osochor. When Young mentions king Siamun he is talking about our Figure 1 king Psinaches, who reigned after king Osochor. But later in this paper, and already in our Figure 1, we identify Siamun as Manetho's king Smendes I, whose reign precedes Osochor by over half a century. If Young is correct in relating the facts cited (and we have no reason to doubt the *facts* he reports) then a king Aakheperre Setepenre preceded the 17th year of king Siamun by several decades, and was possibly, even probably, one of the contestants for the throne vacated by the death of Merenptah in the year 765 BC. Later in this paper we will discuss the

proliferation of aspirants to pharaohic status in the post Santorini era, which would necessarily include Young's king Aakheperre Setepenre.

Having corrected the Wikipedia article, and dismissed Eric Young's discovery as irrelevant, at long last we get to the point we are attempting to make. We agree entirely with the discovery of Jean Yoyotte, that Sheshonk A and Mehtenweskhet did have a son named Osorkon, who became a king. And we do believe the throne names of this Osorkon were Aakheperre Setepenre, not because of Eric Young's discovery, but because of the facts described by Yoyotte, and because there does exist a set of cartouches, inscribed in stone, documenting the existence of a King Aakheperre Setepenre Osorkon, throne names which match no other known pharaoh Osorkon. And for the record, the identification of Yoyette's king Osorkon as Manetho's Osochor is in no way dependent on any determination of his throne names. All that is important is that a king Osorkon can legitimately be added to our Figure 6 as shown, since in the revised history that positioning demands that this Osorkon be identified as Manetho's king Osochor.

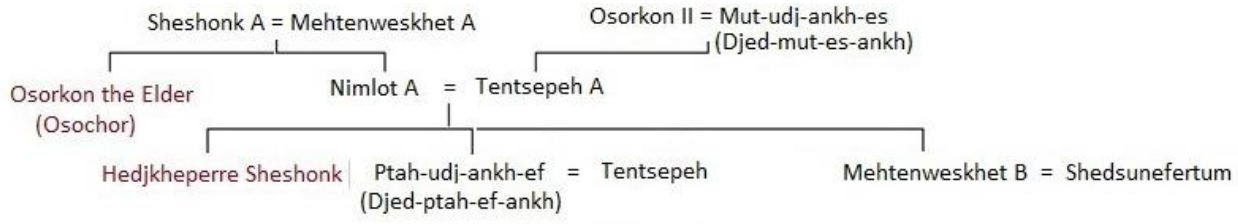
And why do we care if Osochor is the king Osorkon identified by Yoyotte. One reason only. Without the inclusion of Osorkon the genealogy of Pasenhor is entirely concerned with 22nd dynasty kings. The addition of Osochor firmly links the 22nd dynasty and 21st dynasty timelines together. And it links them together precisely as they exist in our Figure 1 set of timelines. That, in effect, proves our thesis that the 20th, 21st Tanite, and 22nd dynasty timelines began around the same time, and it absolutely confirms the positioning of the 21st and 22nd dynasty timelines in our Figure 1.

We remind the reader that we did not create our Figure 1 timelines so that they agree precisely with Petrie's interpretation of the Pasenhor genealogy. They were fixed in place as they now exist by independent arguments long before we discovered the existence of Pasenhor. Petrie's interpretation of Pasenhor's genealogy, especially now with the inclusion of Dodson's Hedjkheperre Sheshonk and Yoyotte's king Aakheperre Osorkon, simply authenticates timelines which were already firmly entrenched in their revised positions. What our Figure 6 does is supply the actual genealogical connections between the kings Osorkon II, Yoyotte's Osorkon the Elder, and Dodson's Hedjkheperre Sheshonk. A glance at

the Petrie genealogy in that Figure 6 informs us that **Osochor** was an uncle of **Hedjkheperre Sheshonk** and that **Osorkon II** was Sheshonk's maternal grandfather. By contrast, the Breasted version presents a radically different version of history, by now thoroughly discredited by a thousand pages of argument in the first three books of our Displaced Dynasties Series. And it follows that the more we can do to verify the accuracy of Petrie's version of Pasenhor, the more certain we become that our Figure 1 is correct.

Thus, before we proceed with our third section of this paper, we add yet another pair of names to the Petrie version of Pasenhor. Already that version, as shown in our Figures 5 & 6, confirms the fact that Nimlot A and his wife Tentsepeh A had a son named Djed-ptah-ef-ankh A married to another Tentsepeh, this in addition to the son Hedjkheperre Sheshonk that we have already added. And Egyptologists, without exception, will confirm the fact that Nimlot A and Tentsepeh A also had a daughter named Mehtenweskhet, married to a priest named Shedsunefertem. This pair of names needs to be added to the Petrie genealogy. The resultant Petrie flow chart, restricted in this case to just the relevant section, appears as diagrammed in our Figure 7 below.

Figure 7: The Petrie version of the Pasenhor genealogy with the addition of the names of Mehtenweskhet and Shedsunefertem.



One further chronological detail needs to be mentioned before we move on. It surely has not escaped the attention of our readers that Petrie's version of Pasenhor agrees not only with the relative positioning of our 20th, 21st Tanite, and 22nd dynasty, but also agrees perfectly with the dates 740-712 BC we have assigned to Osorkon II, dates we determined as early as chapter 3 in our first book *Nebuchadnezzar & the Egyptian Exile*. At no time in our succeeding volumes have we ever ventured to make sense out of the mass of detail recorded by Manetho

concerning the early years of the 22nd dynasty, other than to dogmatically assert that the dynasty must have begun around the year 760 BC, as did the 20th and 21st Tanite dynasties, this because of the 765 BC devastation caused the mega-explosion of the Santorini volcano. We now are able to authenticate that 760 BC date, thanks to Pasenhor, and thanks to Petrie. The reasoning goes as follows.

The reader will first observe that Pasenhor cites precisely three Egypt based Libyan ancestors prior to Osorkon II, the earliest, Sheshonk I (married to wife Karamat), identified by all Egyptologists as the founder of the 22nd dynasty.

We assume that Sheshonk, a Libyan tribal leader, was part of the influx of Libyan refugees fleeing the ravages of the Santorini series of eruptions, those that began in the 5th year of Merenptah and continued for the next several decades. Without fear of contradiction we can date his arrival in Egypt to the approximate year 760 BC. Assuming 1) a father-son relationship between the four earliest 22nd dynasty kings in Pasenhor's genealogy, and 2) that each generation occupied roughly 20 years, and 3) that Sheshonk arrived in Egypt as an elderly tribal leader (roughly 60 years old) with family in tow, all perfectly reasonable assumptions, then we can assume that Sheshonk (I), born around 820 BC, brought with him his 40 year old son Osorkon I (born ca 800 BC), Osorkon's 20 year old son Takeloth (born ca 780 BC), and Takeloth's newly born son Osorkon, the future Osorkon II (born ca 760 BC). We have no quarrel with Manetho assigning 21 years to Sheshonk (I), 15 years to his son Osorkon (I), and an indeterminate number of years, possibly 13, to Takeloth (I), providing we assume that the reigns of the latter three of the four named kings overlapped to some extent that of their fathers, a distinct possibility considering the chaotic conditions that prevailed in Egypt post Santorini.

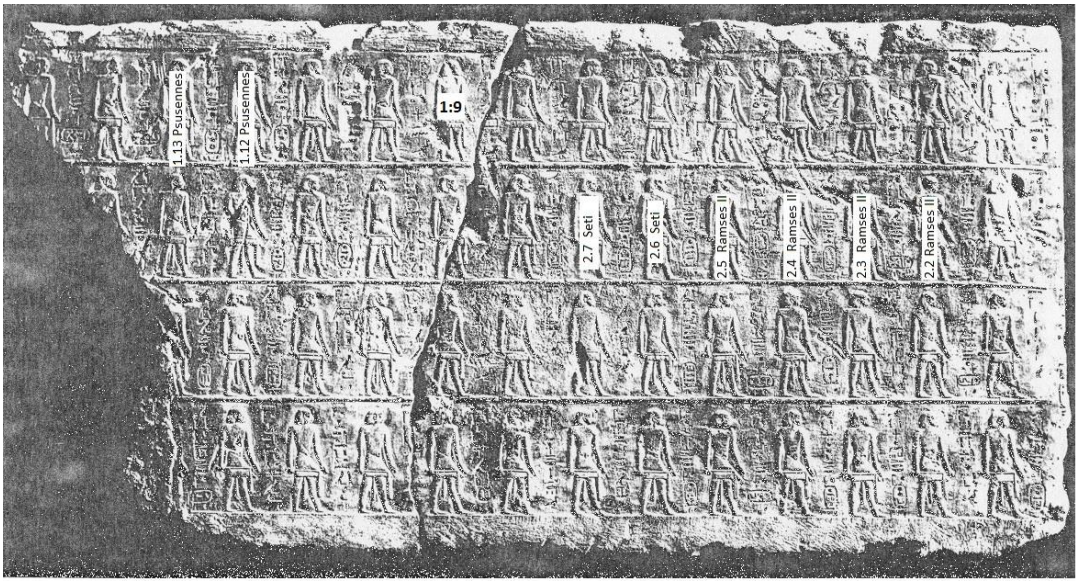
C. The absolute dates assigned to our Egyptian dynasties 20, 21 (Tanite), 21 (Theban), and 22 in Figure 1 are corroborated by multiple sources, including the Berlin and Louvre stele genealogies.

Many times already, and certainly as recently as our last paper, we made reference to the Serapeum stele *Louvre 96 (Cat #52)*, a monument inscribed with the genealogy of a priest named Ashakhet. That genealogy overlaps, in multiple consecutive positions, a sequence of names of high priests/prophets of the Memphite cult of Ptah in Memphis inscribed on a massive monument that now

resides in the Egyptian Neues Museum of Berlin (#23673), hence our reference to it as the Berlin stele. Not all spaces on the Berlin stele contain the names of high priests who served the cult at a particular time, but most do, and many also name a pharaoh under whom they served or with whom they associated. Combining the two documents we were able to create a hybrid genealogy, or minimally a listing of named predecessors of Ashakhet, which we have called the "genealogy of Ashakhet" and which Egyptologists generally refer to as the "genealogy of Ankhefensekhmet", the name of the Memphite priest who apparently commissioned the Berlin monument. From the title of the last two books in our Displaced Dynasties series, both prefaced by the phrase "The Genealogy of Ashakhet", it is apparent that we have depended heavily on the data supplied by this monument in the writing of those volumes.

In Table 2 on page 9 of chapter one in book three we produced a synchronized genealogy, showing where on the Berlin stele we find the names of the ancestors of Ashakhet as featured on the Louvre stele. One of these, a prophet named Shedsunefertem, appears in position 1.9 on the Berlin monument, as indicated by a tag we have added to a photograph of that monument (see Figure 8 below).

Figure 8: The Berlin stele with tags showing the positions occupied by high priests who served under the 21st dynasty king Psusennes I and the 19th dynasty kings Seti and his son Ramses II.



This dignitary Shedsunefertem appeared in this paper in the previous section, where he was identified in Petrie's version of the Pasenhor genealogy as a brother of Dodson's king Hedjkheperre Sheshonk and also of a priest named Djedptahefankh. Therefore, according to our Figure 1, both Shedsunefertem and Djedptahefankh must be dated in the third, fourth, or fifth decade of the 8th century BC, since Dodson's king Hedjkheperre certainly governed the western Delta in the time frame 673-660 BC, and we have conjectured that his reign likely began independently at least a decade earlier, ca 681 BC. We are convinced that Djedptahefankh was the older of the three brothers. His body was found in the Deir-el-Bahari cache DB 320 covered by bandages inscribed by his brother Hedjkheperre Sheshonk (not by king Sheshonk I as erroneously claimed by Egyptologists). And in our point D, which follows, we assign the date range 661-645 BC to Shedsunefertem's position (1.9) on the Berlin stele, which suggests that he was a younger brother of Hedjkheperre.

In Petrie's Pasenhor genealogy Shedsunefertem is a grandson of Osorkon II, whom we have independently dated 740-718 BC. The dates of Osorkon II and Shedsunefertem are thus roughly consistent with the grandfather/grandson relationship assigned them by Pasenhor. And on the Berlin stele we see that two generations of Memphite high priests separate Shedsunefertem's tenure in office, which began in 661 BC, from the last mention of a king Psebkhannu in position 1.12. Since each position on this stele represents approximately 16 years, a king Psebkhannu must still have been reigning around the year 693 BC (661 + 32), absolutely consistent with our dating of Tyetkheperre Psebkhannu to the years 734-688 BC. And for good measure we notice that seven positions separate the time of Shedsunefertem from the last mention of king Ramses II in position 2.2. And since those seven high priests spanned 112 years (7x16), the assumed beginning of the high priesthood of Shedsunefertem around the year 661 BC presumes that the reign of Ramses II ended around the year 773 BC (661 + 112), an excellent approximation since we have previously dated the reign of Ramses II to the years 840-774 BC.

The bottom line in the detail discussed in the preceding paragraphs is this: Petrie's version of the Pasenhor genealogy is absolutely consistent, both in structure and in absolute dating, with the data contained on the Berlin stele and with our timelines in Figure 1.

But before we move on to our next section (D) we need to note, in passing, one set of additional data from the Berlin stele that needs to be explained. We have noted the fact that a king Psebkhannu governed Egypt in the vicinity of the Memphite cult of Ptah during the tenure of priests in positions 1.12 and 1.13 on the Berlin stele. That at most represents a time span of 32 years. It follows that this king, whom Manetho calls Psusennes, and to whom he credits a reign of 46 years, must have also governed additional years partially in both of the time slots 1:14 and 1.11. In spite of that the position 1.14 is assigned to a king whose throne name is translated by Borchardt, the discoverer of the Berlin monument, as Aakheperre Setepnamun. Additionally, position 1.15 is assigned to a king Amenemnisu, and 2.1 to a king whose name is illegible. Thus three names separate the earliest mention of Psebkhannu from the last mention of Ramses II, at least consistent with our independent determination that Ramses II died in 774 BC and Psebkhannu I (Tyetkheperre) began ruling forty years later in 734 BC. Egyptologist are clearly perplexed at this data, but not so our revised history.

What we are most concerned with in this paper is not the absence of the 20th dynasty on the Berlin stele, but the presence of the names Aakheperre and Amenemnisu in positions 1.14 and 1.15 respectively. In the traditional history the reign of Psusennes 1, regardless of whether we identify him as Aakheperre Psebkhannu or Tyetkheperre Psebkhannu, is preceded immediately by a king Smendes 1, identified in that history as Hedjkheperre Nesubanebdjed, and whose reign lasted 26 years. When Borchardt first translated the Berlin stele he would have been astounded, not just by the absence of the entire 20th dynasty, but by the absence of any mention of Hedjkheperre in positions 1.14 and 1.15. In the traditional history there exists absolutely no explanation for this omission of Smendes' name, which should actually have occupied both positions or, minimally, position 1.15. Borchardt "solved" part of the problem by translating the name in 1.14 as Aakheperre Setepnamun, the prenomen of Psebkhannu I in the traditional history, a problematic solution since it assumes that the artisans who inscribed the monument twice used the birth name of Psebkhannu and once his throne name Aakheperre. But that partial solution only made the name in 1.15 more problematic. Amenemnisu is not Hedjkheperre and it is not Nesubanebdjed. Again Egyptologists interject a "partial solution". Some reverse the order of Manetho's second and third 21st Tanite kings Psusennes and

Nephercheres, this because Neferkare had the birth name Amenemnisu; others, like Borchardt himself, who read the position 1.15 name as Amenemopet, place Manetho's king Amenophthis before Psusennes I. In either case the name problem was thus completely solved. Or was it? Now there is absolutely no room for either of the names Hedjkheperre or Nesubanebdjed, especially since the reign of Ramses II was followed by the reign of his son Merenptah, whose name presumably occupied the now damaged position 2.1. Now scholars have to resort to an even more drastic solution to explain the lack of mention of Smendes I. They must conjecture the fact that since the Berlin stele has "inadvertently" omitted the entire 20th dynasty, the name of Smendes I is just another casualty of the faulty construction of that monument. Let the reader decide the merits of that argument.

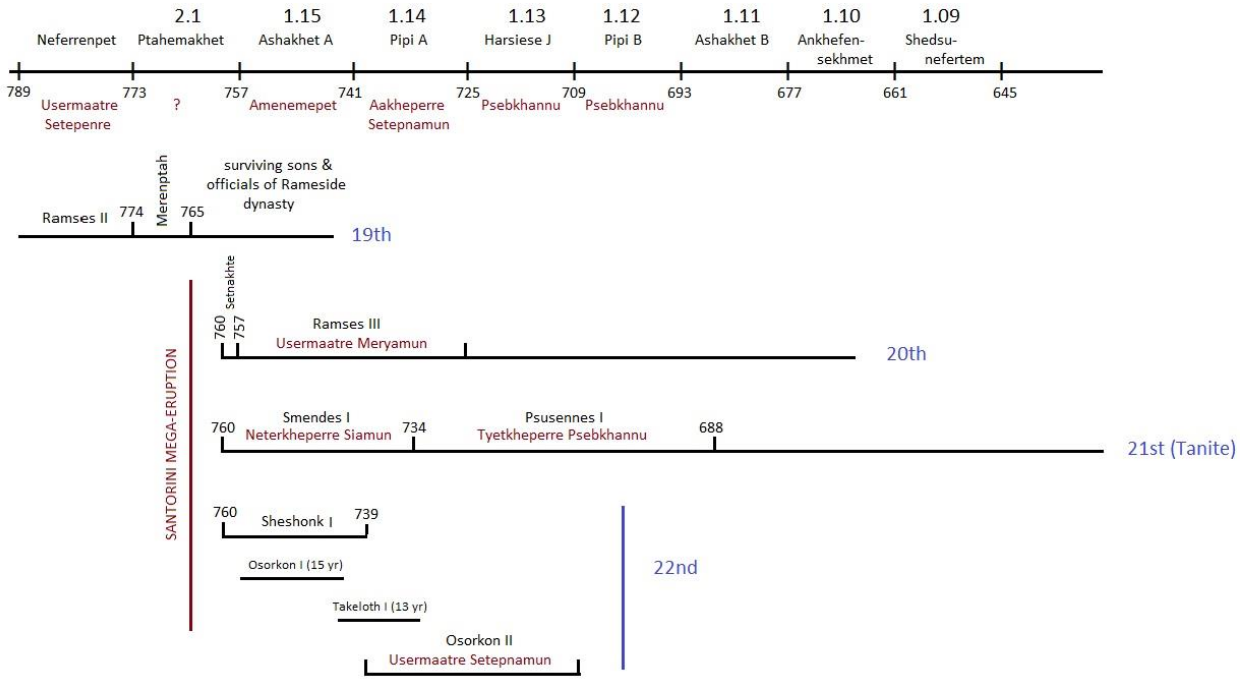
The critic will surely question at this point how our revised history handles the identical problem, that of explaining the apparent absence of the name Neterkheperre Siamun, our choice as Manetho's king Smendes I, the founder of the 21st Tanite dynasty. Fortunately, for the revised history the problem of names preceding the two mentions of Psebkhannu simply does not exist. We have discussed this entire issue already in chapter one of our book three, in particular on pages 15-16. We will not repeat the entire argument here, but we will elaborate in a separate discussion in our point D following.

D. The fact that the name Neterkheperre Siamun does not appear on the Berlin stele can be explained by the revised Egyptian chronology, which indicates the presence of multiple other kings in this time frame who may have been served by the Memphite high priests of Ptah.

Unlike Borchardt, who was almost certainly confused by the absence of the name Hedjkheperre Nesubanebdjed in either of the two end positions (1.14 & 1.15) on the uppermost line on the Berlin stele, we are extremely comfortable with the present selection of names. The difference in expectation is attributable to a single fact. In the traditional history only one of two kings could possibly be mentioned in position 1.14 (either Psebkhannu again or Smendes I), and only a single king (Smendes I) in position 1.15. Alternatively, in the revised history upwards of a dozen possibilities are viable, since we believe that in the time

frames 757-741 and 741-725 BC, represented respectively by position 1.15 and 1.14, the north of Egypt was governed by upward of a dozen kings, many of which have no confirmed cartouche names. We illustrate that point via our Figure 9 below.

Figure 9: A listing of time frames represented by positions 1.9 through 2.2 on the Berlin stele, in association with timelines for dynasties 19 through 22.



Our Figure 9 chart is illustrative only, featuring as it does only the dynastic groups cited by Manetho. But in the preceding paper we discussed the true situation that prevailed in the several decades that followed the Santorini explosion. Not only does the Harris papyrus document the early intrusion of Syrian migrants under the leadership of Rezin, but that same papyrus, plus auxiliary hieroglyphic texts, inform us that mass invasions of Libyan asylum seekers took place in the 5th year of Merenptah (770 BC), the 5th year of Ramses III (753 BC), and again in the 11th year of Ramses III (747 BC), not to speak of the Sea Peoples migrants who invaded in the 8th year of Ramses III (750 BC). We have absolutely no idea how many of the tribal leaders of these groups remained in Egypt and claimed sovereignty over an Egyptian nome/district in the north of Egypt. One group only, led by Sheshonk I, spawned a dynasty recognized by Manetho.

We must also add to this group of possible self-styled pharaohs, a number of individuals with some legitimate claim to the Egyptian throne, most of whom we have barely mentioned in our Egyptian volumes. Not all the offspring of Ramses II and Merenptah perished in the Santorini holocaust. Read any history of Egypt and you will find names of multiple sons of these two kings, as well as several powerful dignitaries, that contested for power following the death of Merenptah. Gardiner, in his *Egypt of the Pharaohs*, cites five – Sethos II, Amenmesse, Siptah I, Siptah II, and the female aspirant Twosre. Monuments exist bearing the cartouche names of these five "kings", each citing regnal numbers ranging from one to 8 years, probably all overlapping in the late pre-Santorini or early post-Santorini era, but it has long been acknowledged that there were other Ramesside contestants, and at least one powerful dignitary, the chancellor Bay/Bey, aspiring for kingship. Most, if not all, of these wanna-be pharaohs were confined to small, regional districts of Egypt, and most, if not all, quickly faded from view during the period 1.15 on our Figure 9 chart. But one, named Amenemepet (or some variant thereof) was apparently acknowledged as pharaoh in this era by the priests of Ptah, and thus apparently governed in the vicinity of Memphis. Unfortunately his identity remains a mystery. If a Libyan, he may well be Sheshonk I, though scholars would do well to search the sons of Ramses II and Merenptah for other suitable candidates. After all, for over a century the priests of Ptah had been associating with 19th dynasty Ramesside pharaohs. They may well have wanted to continue that association into the "new age", even if the adopted pharaoh had extremely limited authority.

As stated earlier, much of this speculation is not new to our revision. On pages 15 and 16 of our 3rd book we discussed the issue, and there we note the varying interpretations of the name in position 1.15 on the Berlin stela. We repeat here one small segment of that conversation, which begins by citing the fact that even the name inscribed in that position is a mystery. It is, in fact

> transliterated Amenemnisu by Kitchen (following Grdseloff and Kees) [Cf. Kitchen TIP 152 n.6 for bibliography] but Amenophthis by Borchardt , who first published the Berlin document [see Ludwig Borchardt, *Quellen und Forschungen zur Zeitbestimmung der Agyptischen Geschichte* (1935) 96-112]. Borchardt transliterated the cartouche name, which unfortunately lies near the broken left end of the inscription, as 'Imn-m-ip-t-rs-t, a considerably different reading than that provided later by Grdseloff and Kees. All three scholars suffer from the same handicap. They are attempting to read a 21st dynasty

> name into the damaged section of hieroglyphs, one believing that the name must refer to Manetho's Amenophthis (identified as Amenemepet by the traditional history), the other Manetho's Nepherkare (Amenemnisu). But the orthography actually resembles neither name as found elsewhere. We believe that the reading Amenmesse Heka-waset may be the correct reading but confess that we are influenced in our judgment by the revised chronology and the desire to read here a name from the period of civil unrest which followed the death of Ramses II. It may well be that the king named here is otherwise unknown to historians. It could be one of Ramses many son's, some of whom no doubt contested for power but left no other record of their existence. We note that Ramses did have a son named Amenemopet with orthography close to what is visible in the inscription. Perhaps Borchardt was correct in the reading but wrong in the assignment of the name. We leave the matter there. [Book 3, chapter 1, pp. 15-16]

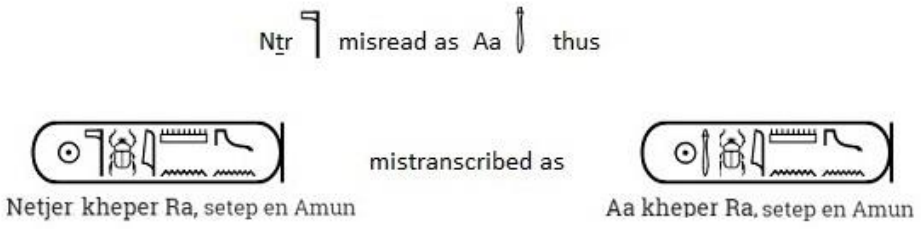
So much for the name in position 1.15. We actually did not expect to see there the name of our king Smendes – Neterkheperre Siamun. The reason is simple. In our previous paper we assumed that Smendes, in the aftermath of the Santorini explosion, was the High Priest of Amun (HPA) in Thebes, and as HPA he was also the commander of the Egyptian army in the south. His counterpart in Tanis, assuming there was one, was likely killed in the aftermath of Santorini, whether by the elements associated with volcanism, including tidal waves, or by invading migrants. Early on, perhaps at the behest of Setnakht or Ramses III, Neterkheperre travelled north to combat the waves of advancing Libyans and Sea Peoples. At the time, and for much of the time frame 757-741 BC represented by the Berlin stele position 1.15, he remained simply a high priest and army commander. Only near the end of that time frame did his political aspirations change, and he assumed cartouche names. We are not guessing. At least one document exists which confirms this thesis (see below in our section E).

But it we are correct in our theory of 21st dynasty origins, the reader may still legitimately raise the question of why the Memphite priests did not begin to embrace the new Tanite king as he emerged in the 1.14 time slot. One possible answer has already been mentioned. Chaos still reigned supreme in Egypt. Perhaps the priests of Ptah determined to continue their association with Ramesside kings, or side with the newly arrived Libyan 22nd dynasty descendants of Sheshonk I (assuming they had begun this association in the earlier time frame). Both Osorkon I or Takeloth I are possible claimants for the prenomen Askheperre Setepnamun. Or is it possible that the Memphit priests did actually

begin to recognize and associate with the newly declared pharaoh in Tanis? Is it possible that the name of the pharaoh in position 1.14 has been misrepresented by Borchardt, and confirmed in that misrepresentation by multiple other Egyptologists in the generations since his publication of the Berlin stele inscription? Should Borchardt's transcription of the name in position 1.14 be changed from Aakheperre Setepnamun to Neterkheperre Setepnamun, the full prenomen of our king Smendes I?

Of course we would not pose the question if we did not expect an answer in the affirmative. Keep in mind that we are dealing here with a 2600 year old inscribed piece of stone, buried for much of that time, but damaged nevertheless, and particularly near the extremes. Keep in mind also that Borchardt expected to read one of two names in position 1.14, either the name Hedjkheperre Nesubanebdjed (which was a non-starter) or the name of Aakheperre Psebkhannu. We will not accuse him of bias. He was, after all, a highly skilled and reputable scholar (in spite of the fact that his Nefertiti statue has received critical revues, suspected of being a forgery). But if faced with a decision as to how to read a particular hieroglyph, particularly in a worn-out/damaged section of a 2600 year old monument, we cannot fault him for producing a reading consistent with the traditional history he espoused. And the readings Aakheperre and Neterkheperre, quite distinct in English translation, differ by a single character in the Egyptian hieroglyphic text. And the two competing hieroglyphs, if at all weathered by the elements, would be virtually indistinguishable. Thus we propose, as one possible solution of the problem of names in position 1.14, that Borchardt has read the hieroglyph "Netjer/Ntr" (Gardiner's Grammar sign #R8) as if it were an "Aa" (Gardiner's Grammar sign #O27). See our Figure 10 below for clarification

Figure 10: Explanation of how the pharaonic name in position 1.14 on the Berlin stele was mistakenly read by Borchardt as Aakheperre rather than Neterkheperre.



If we are correct in our assumption that the king's name in position 1.14 is that of Neterkheperre Siamun (and we emphasize the *if*), then we can safely rest our case. Smendes I is Siamun. Psinaches must be Hedjkheperre Nesubanebdjed. The 21st Tanite dynasty did not begin in the days of Herihor and Piankh. It began a century earlier. Our Figure 1 is entirely correct. Petrie's version of Pasenhor is correct in every detail, especially as augmented with the names of Seshonk and Osochor. And the Berlin stele chronology is 100% reliable, assuming we have correctly determined its internal chronology.

E. Assigning the correct cartouche names to Smendes absolutely confirms the fact that this 21st dynasty founder lived at the same time as Ramses III, thus authenticating our Figure 1 timelines.

We use this final section not only to provide further argument equating Siamun with Manetho's Smendes, but also to summarize our finding throughout this paper. In our opening statement on page one we suggested that a secondary purpose of this paper was to supply cartouche names for all of the named kings on the 21st Tanite dynasty timeline in Figure 1. Prior to the publication of this paper we had deemed it sufficient to simply list the seven Greek names precisely as passed down to us by Manetho, a 3rd century BC Greek speaking Egyptian priest. Two of Manetho's names were already sufficiently clear in previously published lists that we considered it sufficient in Figure 1 to simply replace, without comment, Manetho's *Nephercheres* with Neferkare Amenemnisu and Manetho's *Amenophthis* with Usimare Amenemope. We then singled out for attention Manetho's two kings by the name Psusennes and the king Psinaches, supplying the true names of these kings, identifying Psusennes I as Tyetkheperre Psebkhannu, Psusennes II as Aakhepere Psebkhannu and Psinaches as Hedjkheperre Nesubanebdjed. The accuracy of those names were defended in the first section of this paper. That left only two of Manetho's kings to identify.

In the course of writing the second section we were favored by Egyptian scholars with the identity of Osochor, aka Osorkon the Elder, aka Aakheperre Osorkon, leaving us with but a single unidentified king – Smendes (I) the founder of the dynasty. Many times in the course of our revision of Egyptian history we noted the fact that Smendes, for going on 200 years, had been incorrectly identified as

the king Hedjkheperre Nesubanebdjed by 18th and 19th century scholars, and our transference of his former cartouche names to Psinaches left Smendes bereft of both birth and throne names. We now set the matter straight. Scholars have for over a century been aware of the existence of a 21st dynasty king named Nuterkheperre (or Neterkheperre) Siamun, and debate has continued over the centuries as to which, if any, of Manetho's 21st dynasty names he should be associated. One popular choice was Psinaches, but that spot is now filled. And the only remaining possibility is Smendes. And so, without hesitation, we identify Smendes as Nuterkheperre Siamun. This is not, as it may seem, an afterthought. We have long considered these to be the true cartouche names of Smendes, and stated as much in the first chapter of our third book, a book written over a dozen years ago. We are not guessing. We cite below five reasons for our selection, beginning with the obvious fact that

1. After identifying Manetho's other six kings, Smendes is the only king remaining to be identified, and Nuterkheperre Siamun is the only remaining unassociated king. Let the reader draw the obvious conclusion.

To which we add the following four reasons:

2. The king names **Sm**e**n**des and **S**ia**m**u**n** are so remarkably similar that they almost demand association, especially in the ancient world where consonants were king and vowels were optional in written texts, Egyptian texts being one prominent example.

3. When we identify Nuterkheperre Siamun as Smendes I, we are not in conflict with any documentary evidence to the contrary. The inscriptional landscape regarding Smendes is a *tabula rasa*, a blank slate, assuming of course that scholars insist on looking for documents from this time frame citing the name Hedjkheperre Nesubanebdjed. There are none. According to Kenneth Kitchen:

> From the reign of a quarter of a century, hardly any monuments have so far been recovered that explicitly name the new pharaoh [Hedjkheperre Nesubanebdjed] himself in their datelines. (TIP sect 213 p. 255). [bracketed addition supplied by this author].

In Kitchen's massive volume (*The Third Intermediate Period in Egypt (1100-650 B.C.*) 2nd ed. with supplement), consisting of almost 600 pages of small print

heavily detailed notes, this authority on the *3rd Intermediate Period* could only find material enough on Smendes I to fill two pages of text (pp 255-257). And most of that material has absolutely nothing to do with Smendes. Kitchen references a long series of year dates on burial items and graffiti *without royal name* (that are almost certainly attributable to Aakheperre Psebkhannu), and he attempts to draw some conclusion from the so-called "Banishment stele" (aka the Maunier stele), which we discussed earlier in this paper and which we analyze at length on pages 273-279 in our book two, an inscription which also has absolutely nothing to do with Smendes. The year 25 mentioned in that stele belongs to Pinudjem I, not Smendes, and it references Pinudjem's death and the ascendancy of his son Menkheperre, who proceeded immediately to drive the Assyrians from Egypt and to free the hundreds of Egyptians banished to the eastern desert decades earlier by the Assyrian kings Esarhaddon and Ashurbanipal. The year this took place, according to our revised history, was 637 BC, not the end of the 11th century BC.

The only other subjects discussed by Kitchen, indeed his primary focus in the several pages of his text, are the travels of Wenamen and the Dibabieh stele, both of which we have previously dismissed, arguing earlier in this paper that both sources are supportive of our revised history, not of the traditional association of the names Nesubanebdjed and Smendes.

4. We saw in our previous point D discussion, in our analysis of the Berlin stele, that the name in position 1.14 on that monument, immediately preceding the dual mentions of a king (Tyetkheperre) Psebkhannu, was definitely not that of Hedjkheperre Nesubanebdjed, nor of Psebkhannu, but possibly belonged to Nuterkheperre Siamun, thus confirming our identification of Smendes I.

And finally,

5. In our previous section D we explained the absence of the name Nuterkheperre Siamun in position 1.15 as a result of this king's lack of cartouche names in the first decade of his tenure in Tanis, and his role as military commander of the Egyptian army in the south of the country, moving north to assist Ramses III in combatting the Sea Peoples Libyan intruders. We discussed some of these facts also in our previous paper when they arose as part of our

discussion of 21st dynasty origins. While we cannot absolutely prove that the 21st dynasty began with the military intervention of Smendes, at the time the HPA in Thebes, we can at minimum substantiate two aspects of that theory.

On the one hand there is absolutely no doubt that the 21st Tanite kings were first and foremost High Priests of Amun in Thebes, and commanders of the Egyptian army, and only secondarily Tanite pharaohs. We need go no further than the reign of Tyetkheperre Psebkhannu, the successor of Nuterkheperre Siamun, to establish that fact. A glance at the Wikipedia article related to this king Psebkhannu provides sufficient proof of those facts, especially one statement in the opening dialogue:

> The Egyptologist Karl Jansen-Winkeln notes that an important graffito from the Temple of Abydos contains the complete titles of a king *Tyetkheperre Setepenre Pasebakhaenniut Meryamun* "who is simultaneously called the HPA (i.e., High Priest of Amun) and supreme military commander."[4]This suggests that Psusennes was both king at Tanis and the High Priest in Thebes at the same time, meaning he did not resign his office as High Priest of Amun during his reign.

As for Smendes' participation in the battles with the Sea Peoples, alongside Ramses III in that king's 8th year, we need only turn our attention to an inscription on a badly damaged building south of the main temple of Amun in Tanis, cited by Kenneth Kitchen in his TIP (sect. 235, pp 280-81):

> At this point, it is apposite to cite (as others have done) a fragmentary relief of Siamun from a thoroughly-destroyed building which had been erected by Psusennes I and Siamun, east of the royal tombs and just south of the main temple of Amun in the great precinct of Tanis. This relief shows Siamun in the pose of smiting with uplifted mace a group of prisoners who grasp a double axe of a type reminiscent of the Aegean and West Anatolian world. A merely conventional temple-scene of this king would of itself prove nothing, and least of all that the pharaoh had ever actually gone to war. But such reliefs were commonly carved under kings who did, and here the detail of the very special form of axe-head suggests that this relief was a commemoration, in traditional 'theological' form, of a real campaign against the Philistine and Sea-peoples population in South-West Canaan.

The reader needs to remember, when reading Kitchen's comment, that when he references king Siamun he is referring to Manetho's king Psinaches. But apart

from that errant association we agree entirely with absolutely everything in the above statement, save for Kitchen's suggestion that the wall mural created by Siamun is merely a "commemoration" of some earlier battle conducted by some other pharaoh, a "stretch" to say the least. We argue instead that Siamun created these reliefs because he himself fought against the Philistines and other Sea-Peoples in the approximate year 750 BC, a fact which confirms, as does no other inscription, that Siamun must be identified as Smendes 1, not Psinaches. We understand completely Kitchen's reluctance to accept the fact that Psinaches fought battles against Sea Peoples, a chronological impossibility. But for our revised history, which believes that Siamun must be dated in the time frame 760-734, that he must be identified as Smendes I, the founder of the 21st dynasty, and that he was a contemporary of king Ramses III and almost certainly fought against the Sea-Peoples in his capacity as commander in chief of the Egyptian army, this fragmentary relief discussed by Kitchen is a god-send. We might well have simply included it on our page one, and gone on to our next paper.

Needless to say, Kitchen's analysis of the Tanis relief attributed to Siamun has encountered resistance within the community of Egyptologists. Thus in later years Kitchen defended his TIP remarks in an article entitled *Egyptian Interventions in the Levant in Iron Age II,* on pages 113-132 of an anthology entitled *Symbiosis, Symbolism, and the Power of the Past*, eds. W.G. Dever & S. Gitin ASOR (2000). We end this paper by quoting, on the following page, Kitchen's remarks on pp 118-19 of that article.

2. Siamun and the Levant. Here we plumb new depths of hypercritical overkill. From Tanis, home base of the 21st Dynasty, there has long been known part of a triumphal relief scene showing a king—agreed to be Siamun—smiting captured foes. First of all, astonishingly, Weinstein (1998: 192–93) has dismissed this scene as showing "an unidentified king." Unidentified? Before this blundering remark is taken up by biblical scholars who may not know one hieroglyph from another, let the facts about this piece be stated clearly once and for all. In front of the king's face there appears for all to see a cartouche containing the personal name and epithet "Siamun, Beloved of Am[un]." All of the signs are legible and complete, except for loss of the second Amun's head and plumes at the top right. Numerous other intact examples of Siamun's nomen guarantee the reading absolutely. A trace of the first cartouche, adjoining at right, would have contained this king's unique and very distinctive throne name, Neterkheperre Setepenamun.[17] Second, the dating of Siamun: as amply demonstrated elsewhere (Kitchen 1996a) and summarized in excursus 1 below, this king reigned for 19 years, from 979 to 960 or 978 to 959 B.C.E., or very close to this. Third, the object held by his foe in this scene: despite much nonsense written to the contrary, this is very clearly a *crescentic* double-bladed *ax*—not a shield (Lance 1976: 216–17; Green 1978: 364), a halter (Ash 1999: 45), or still less, handcuffs (Ash 1999: 45).[18] Such a weapon in this precise form is not found in the armories of Nubia, Egypt, Libya, Syria–Palestine, Mesopotamia, or Anatolia; but doubled-bladed axes (real or ceremonial) *do* occur prominently in the Aegean cultures and in the Balkans.[19] From across the Aegean and eastern Mediterranean came the "Sea Peoples," including the Pilasti or Philistines. Therefore, whether as weapon or symbol, the ax is appropriate for the first major population group that Siamun would meet as he marched into southwest Palestine.

The ax is clearly marked as such by a visible socket with a trace of the handle running down from it onto the now-lost lower wall surface.[20] The foe's hand is clearly visible, holding the ax awkwardly at the socket, so that he cannot wield it against the king.[21] Such a feature is unique in the long series of triumph scenes and implicitly speaks for its commemorating a historical event. It should be remarked that, as far as can be determined, the *monumental* examples of such scenes on temple walls do seem to belong to pharaohs who had gone to war. Thus, we find such scenes of the 18th Dynasty warrior-kings, of Sethos I, Ramesses II, and Merenptah in the 19th Dynasty, and of Ramesses III and Ramesses VI in the 20th Dynasty; thereafter, only of such kings as Siamun, Shoshenq I, and Shabako/Taharqa before the Late Period. Examples in purely decorative contexts (on the sides of sacred barges, in jewelry contexts, etc.) are merely ideograms of victory in the abstract,[22] not to be confused with real records on temple walls, where triumph scenes often form the terminal scene of those showing real campaigns (19th/20th Dynasties) or substitute for them (18th Dynasty). Thus, the whole tissue of arguments against the probable historical significance of Siamun's relief can be dismissed as the special pleading it in fact is.